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HALF-YEARLY ABSTRACT
OF THE
MEDICAL SCIENCES.
JULY—DECEMBER,
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THE
HALF-YEARLY ABSTRACT
OF THE
MEDICAL SCIENCES:

BEING
A PRACTICAL AND ANALYTICAL DIGEST OF THE CONTENTS OF THE PRINCIPAL
BRITISH AND CONTINENTAL MEDICAL WORKS PUBLISHED
IN THE PRECEDING SIX MONTHS:

TOGETHER WITH A
SERIES OF CRITICAL REPORTS ON THE PROGRESS OF MEDICINE AND
THE COLLATERAL SCIENCES DURING THE SAME PERIOD.

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HALF-YEARLY ABSTRACT

OF

THE MEDICAL SCIENCES,

&c. &c.

PART I.

PRACTICAL MEDICINE, PATHOLOGY, & THERAPEUTICS.

SECT. I.—GENERAL QUESTIONS IN MEDICINE.

(A) HYGIENE.

ART. I.—*An instance of the salutary effects of simple Sanitary Measures.*
By the REGISTRAR-GENERAL.

(Quarterly Return of the Registrar-General, Oct. 28th.)

“FOLLOWING the waters of the hills of Buckinghamshire down through the fens we arrive at Ely. Here a remarkable example is found of the salutary effects of simple sanitary measures, of which every town in the kingdom may have the advantage. Ely stands, with its lofty cathedral, on one of the old fen islands. It is a small city of 6176 inhabitants (in 1851), and is in the neighbourhood of the low lands, where the great systems of modern embankments and draining were commenced by Vermuyden, one of Cromwell’s colonels of horse. The Bishop of Ely in ancient times went in his boat to Cambridge. And the country around, like all our old marshes, is still imperfectly drained. The atmosphere has therefore no natural advantages. The Public Health Act was introduced in 1851. The Ely Board of Health was founded. They set on foot two great works; one for supplying the town with water, the other for carrying off that water through every house clear out of the town. The public works were completed at the end of 1854; and the houses were gradually connected with the public sewers, leaving, however, at the end of 1857, 200 in 1200 houses out of connexion. Mr. Marshall, the superintendent-registrar of the district, in an able paper shows the result of this great experiment. In the seven years

(1843-49) before the Public Health Act was in operation the mortality was at the rate of 26 deaths annually to every 1000 living; in the seven subsequent years (1851-57), when the sanitary measures were only partially carried out, the mortality fell down to the rate of 19 deaths annually to every 1000 living. The mortality in the last two years (1856-57) was at the rate of 17 in 1000. In the same periods the surrounding rural parishes underwent some improvement; but the improvement of the city has advanced so much more rapidly that its mortality was in the last two years 4 in 1000 less than the mortality of the surrounding country. The young people under the age of 35 have enjoyed remarkable immunities from disease, and the benefit will be transmitted to succeeding generations. The two chief sanitary works which have been completed are the introduction of water taken from the river of inferior quality, and the destruction of 4000 cubic yards of cesspools—nearly four yards to each house. The surveyor, Mr. Burns, exclaims, with justifiable pride, ‘There is still a number of cesspools remaining, and the sooner they are done away with the better. After this is done, I may truly say that I found Ely a city of cesspools, filth, and sickness; but I shall leave it a city of drains, health, and cleanliness, and that is something to be proud of.’ Yes, Mr. Burns, you may well be proud of your work. Pau, in the Pyrenees, to which British invalids still resort for health, experienced a mortality of 28 and 23, when you had reduced the mortality of Ely to 17 in 1000.

“The citizens of Ely have sunk 15,000*l.* on their sanitary works, which appear to have been conducted in something like the same determined spirit as animated Cromwell’s colonel of horse. Certain rate-payers who enjoy the benefits complain of the burthen of the rates.

“We may now turn from this small resolute city in an agricultural district to Liverpool, the second city of the kingdom, where the great work of sanitary improvement is also advancing.

“Liverpool lies on the shore of the fresh, deep, wide Mersey, which is lined by her magnificent docks, and the houses rise from the river over the red sandstone heights in long stretching lines. Lancashire and Cheshire cover the plains and hills behind the queenly town; before her are Ireland, America, and the ocean which her ships ride over, carrying foreign produce or the manufactures of the north to and from the various regions of the world. The place is well chosen for health; and in ‘Gough’s Camden’ (ed. 1806) Liverpool is said to be celebrated for ‘her beauty and populousness.’ Yet it was found and published in the first Registration Report that the mortality of the population of this district was in the latter half of 1837 at the rate of 39 in 1000 annually; while the population of West Derby, containing, with other parishes, the outlying parts of the borough, died at the rate of 25 in 1000. The strong contrast between the contiguous places and the subsequent discovery that Liverpool was one of the unhealthiest parishes in the kingdom, excited surprise as well as regret in the public mind. A local association was formed of some of the best people; and the causes of the mortality were investigated, and were afterwards made known by Dr. Sutherland in a series of lucid papers. Little, however, was done; the prophets had been crying in the desert; and in 1846 the mortality grew still more threatening. Cholera raged fearfully in the borough in 1849.

Steps were now taken to carry out sanitary measures, under the direction of Dr. Duncan and the other able officers of the town. The sanitary school of Liverpool subsequently furnished some of the most efficient members of the commission which did good service in the East. Still it is to be regretted that the health of the great bulk of the population has improved but slowly. Liverpool has a good supply of water; but it is still infested by cesspools, including under this name the filthy Lancashire midden; and the drains pour their contents into the dock basins, which exhale a malarious sickly air over the people. The mortality in the borough of Liverpool was at the rate of 29 in 1000 in the year 1857. Much good, therefore, has been done since 1857; thousands of lives have been saved. Still Liverpool has not yet, like Ely, taken the bull by the horns. Or why should not the mortality be as low as 19 or even 17 in 1000? What natural advantages has Ely, taking one thing with another, over Liverpool? If Ely has had thousands of cubic yards of dirt removed, Liverpool has hundreds of thousands of cubic yards to deal with; but her means are commensurate with her duty. Mr. Newlands can do for Liverpool what Mr. Burns has done for Ely. The cesspool, the midden, or call it what they may, for it is still the same, is the chief destroyer of the Lancashire population. Crowded dwellings, vice, want, do a part of the mischief; but in Liverpool the cesspool destroyed a large proportion of the 6418 people who last year perished in excess of the numbers who would have died at the rates prevailing in country districts. The tender-hearted may shed natural tears over them as they lie in the cemetery. Abolish the cesspools of Liverpool, and you immediately save the lives of thousands of people. Yet the parties who have exerted themselves to put a stop to capital punishments have not been roused by the ruthless destruction of men; and no Beccaria has written on these, crime, and punishments. A living poet, in one of his last poems, exclaims—

‘ Ah, it is the gallows tree !
Breath of Christian charity,
Blow ! and sweep it from the earth.’

“ But what number of lives did the ‘gallows tree’ take away in 1857? 13 in all England and Wales; two only in Lancashire. And these were the lives of murderers, who were put to death for their crimes after the most deliberate judicial inquiry. But the 6418 men, women, and children of Liverpool were destroyed cruelly in that year without discrimination. Of the cesspool, rather than of the gallows tree, a wiser muse will sing—‘Sweep it from the earth.’ ”

ART. 2.—*Adulterated and unhealthy Milk.* By Dr. ROUTH, Physician to the Samaritan Free Hospital for Women and Children.

(*British Med. Journal*, April 5, 1858.)

Cows’ MILK.—This is the substitute for human milk best known in these regions. The absence of odour and its more general diffusion, are advantages in its favour. In appearance it is of a bluish-white colour, almost tasteless, specific gravity varying from 1.030 to 1.035. Its microscopical characters are about the same as those of human milk, excepting

that the milk-globules are more abundant. Now, it is clear, comparing this with woman's milk, that—1, the quantity of water is less in the cow; 2, the solid matters are in greater quantity; 3, the sugar is less in amount; 4, there is more casein; 5, and more butter; 6, the salts are also in excess.

It is quite manifest that a simple dilution of this milk will not suffice. This may be added to diminish the relative quantity of casein and butter to the normal figure it attains in human milk; but it will only reduce unduly also the amount of sugar; and thus, at the outset, we meet with a difficulty in its employment. But there are other difficulties more serious and difficult to contend with, and which tend to affect materially its quality. A few of these I will consider *seriatim*. They are—1, adulteration of cows' milk; 2, its acidity, dependent upon stall-feeding; 3, the effect produced upon the milk by feeding cows in a proper manner; 4, the effect upon milk of keeping cows in unhealthy sheds.

Adulteration.—The most painful part of our experience in town is, that pure milk cannot be procured; it is almost always adulterated. In the excellent work of Becquerel and Vernois, the '*Annales d'Hygiène*' (and in this respect Dr. Hassall confirms their results), it appears to be adulterated in Paris by the following substances: water, glucose, flour, starch, dextrine, infusion of amylaceous matters (rice, barley, bran), grumous matters, yolk of egg, and white of egg; sugar, gelatine, liquorice, boiled carrots, broken-down calves' brains, serum of blood, several salts, bicarbonate of soda, chalk, turmeric, emulsion of hemp or almond seeds, &c. We do not, however, find that most of these are commonly employed. The adulteration by water is, however, extensively practised in England. Dr. Hassall, out of 26 samples of milk, found that 11 were adulterated with water in the proportions of from 10 to 50 per cent. Dr. Sanderson, the medical officer of health for Paddington, found in 32 specimens of the milk examined by himself and Mr. Alfred Bernays, of St. Mary's Hospital, that in all, except one, the quantity of water was greater than what it was in pure milk. In many instances (12 times), the quantity of solid constituents was only half as great as it should be, in a few only one fourth; many specimens containing less than 6·5 or 5·8 per cent., in a few gradually diminishing to 3·5 instead of 12·98 as in pure healthy milk.

Dr. Hillier, the medical officer of St. Pancras, examined 20 specimens of milk, and found that the quantity of water added varied from 25 to 50 per cent. That supplied to the workhouse was one of the poorest. Instead of a gallon containing nearly 9000 grains of *solid* matter, it contained only 5425 grains, or two thirds the proper quantity. Dr. R. D. Thompson found in Marylebone, that the gallon of milk, in seven samples, weighed as a mean 71,680 instead of 72,415 grains, which amounts to the withdrawal of 1·44 oz. of solid matter, well calculated to nourish the body, and substituting for it water. Dr. Hyde Salter and Mr. Hunt, from the confessions made to them by milkwomen, their patients, state the quantity of water usually added is one gallon of water to two of milk. What sort of food can this be for an infant, especially if diluted as it almost invariably is by the purchaser, and often afterwards by medical direction? Is it to be wondered at that children fed on such weak milk do not thrive?

Acidity.—Cows' milk, except the animal has been fed upon grass exclusively, is almost always *acid* in stall-fed cows; human milk is always alkaline; hence another reason why cows' milk disagrees with many children.

The experiments of Dr. Mayer of Berlin are particularly conclusive upon this point. He says for a considerable time he had been in the habit of examining the milk of every householder in Berlin, and testing it by litmus paper, according as the cows were fed from brewery slops or brandy lees, gardeners' produce, or in the country. In every instance, except one, he had found the milk decidedly sour. *a.* Of cows fed with brewers' lees, red potatoes, rye bran, and wild hay, in five instances the milk was slightly sour, in one very much so. *b.* Of forty cows fed with potato mash, barley husk, and clover and barley straw, in ten which were examined the milk was sour, in three very sour. *c.* From among fifty cows, fed on potato husks, barley husks, and wild hay, five were examined, and in all the fresh milk was sour. *d.* From fifty-two cows, fed on potato mash, husks, wild hay, rye straw, out of twelve selected for examination the fresh milk of all was sour. *e.* From six cows, fed by a chief gardener on coarse beet-root, red potato, bran mash and hay, the fresh milk was faintly sour. *f.* From five cows, fed by a cow-feeder on lukewarm bran mash, and hay, in four the fresh milk was quite neutral, in one it was decidedly alkaline. The whole of these experiments were made in the winter season, when cows were necessarily stall-fed, and confirm the truth of the general opinion, that the fresh milk of stall-fed cows is almost invariably acid. Dr. Mayer does not believe that this acidity is due to want of exercise, so much as to the unscientific manner in which the cows are fed; and he particularly objects to the potato mash, which he considers the cause of this acidity. The milk of the cows of gardeners and cow-feeders is usually praised by the Berlin women as being particularly good. But Dr. Mayer has observed that it often gives rise to diarrhoea and cutaneous eruptions in children; which, he supposes, is due to the cows being fed with the cabbage, turnip, and potato refuse. The very worst milk is that supplied by cows fed on potato refuse from brandy distillers, as opposed to that obtained from the cows of cow-fatteners, which feed on hay and grass in stalls. By substituting the milk of the latter for the former, he is often enabled to arrest at once the intestinal derangements previously referred to.

Effect upon cows' milk of various kinds of food.—It must be admitted that a great deal depends upon the manner in which cows are fed. Generally this is done in the cheapest possible way, because milching cows so deteriorate in value after eight or nine months' use as such. I am told that a cow purchased for 18*l.* to 20*l.* at the beginning of a season, will sell at a loss of 6*l.* or 8*l.* at the end of it; they look so small and meagre. But this may easily be prevented. A very intelligent gentleman in Nottinghamshire has informed me that if the cows are fed upon a steamed food composed of chopped hay, bran, malt calms, and rape-cape, not only will they produce an extra quantity of milk, but keep throughout the milching period in first-rate condition; in fact, they will at the end of the six or nine months' milk look as well as they ever did.

Country milk and town milk.—The former is stated to be preferable

to the latter. The reason is, no doubt, that the cows are less crowded together, and the milk is less watered. Becquerel and Vernois have also proved the truth of this popular opinion from their experiments.

This is not, however, due to the mere exposure to country air, because experiments have been made, and when the cows are fed on hay, with oats or barley-straw, the ordinary culinary roots with a certain quantity of wet bran, a similar result is obtained in towns.

Summer and winter milk.—Owing to the difference of nutriment given, the composition of these two milks is not the same. The principal difference observed in winter is a diminution of the water, and among the solid constituents an increase of the butter only; both the casein and sugar are slightly diminished. In summer there is more water; but what is remarkable is, that among the solid constituents the casein, sugar, and salts are diminished, and the butter is considerably increased.

Results obtained by various kinds of food.—Dr. Playfair adduces an example of a cow fed on much nitrogenous matter, in which not only was the amount of nitrogenous matter or casein in the milk increased, but also the butter. Certainly the yield of milk is increased by much of that stimulant diet which is occasionally given to cows, such as refuse slop from whisky distillers, which is known to be given largely in America, and for which cows acquire so depraved an appetite that they will not take afterwards their ordinary food. Other less exciting food has the same result. Thus, Parmentier and Deyeux found that cows fed on the leaves and stalks of maize yielded more milk than when fed on ordinary fodder. Moreover, the milk was extremely sweet. The milk obtained from cows fed on potatoes and common grass was much more serous and insipid. That from cows fed on cabbage was much more disagreeable to the taste. Hermanstadt found also that fresh aliments caused a larger quantity of sugar to appear in the milk than dry food.

Among the most approved fodders for cows are sainfoin, Spanish and ordinary trefoil; but there are a vast number of other annual plants chosen from among the graminaceæ or leguminosæ, which, if cultivated and given to the cows, would prove exceedingly useful. Indeed, Anderson assures us that he had seen cows fed upon trefoil and grass which yielded a superior kind of butter to that afforded by cows fed upon famed old pasture. The ancient faculty of medicine in Paris appointed a commission in 1771 to trace the effects of various roots on the milk of cows. These reported the potato to be particularly useful in increasing the quality and the flow of milk; and that its administration to mothers of thin weakly children had led to the rapid improvement of these latter in every respect.

Effect on milk of keeping cows in unhealthy sheds.—The supply of good and selected food is, however, only one part of the management needed to ensure good milk from milk-bearing animals. Excessive cleanliness should in every way be enforced. Upon the subject of the cleansing of cow-sheds, Messrs. Parmentier and Deyeux remark—Nothing contributes more to maintain the good quality and quantity of cow's milk than scrupulous cleanliness in the shed. If the fæcal matters are left about and removed only at long intervals, the cows lying amid all this mess are always weak; the udders are hot; and the milk, so susceptible of acquiring a bad odour, soon contracts the bad taste, from which it is with

difficulty again deprived. The great reputation of the cows of the Prevalaye is due to the remarkable cleanliness in which they are kept, which also enables them to yield an abundance of milk, and to be particularly innocuous to disease.

There can be no doubt, from the general foregoing remarks, that if the subject were more closely studied, cows and goats might be so cared for and so fed as to yield a quality of milk which would be found most serviceable to children brought up by hand. The milk obtained from cows fed upon beet-root, with a very small dilution of water, might be brought so closely to resemble human milk as in all respects to perform the same services. But every day's experience proves that nothing but the most stringent measures can effectually remedy the abuses that prevail. Parliament must interfere, and in what better cause could it do so than by compelling all cowkeepers to sell good milk, to strengthen the bone and sinew of its people, and preserve the lives of thousands of helpless babes? Till this is done, our best efforts, it is feared, will prove nugatory.

ART. 3.—*On vegetable substitutes for Human Milk.* By Dr. ROUTH, Physician to the Samaritan Hospital for Women and Children.

(*Med. Times and Gazette*, Aug. 21 and Aug. 28, 1858.)

One would have thought that a very little reflection would have convinced any observer, that if among herbivorous mammalia the young require animal food, that is, *à priori*, a strong argument against the use of vegetable food; yet even upon this point our medical authorities are not agreed; many vegetable compounds are both recommended and taken. Apart from this common-sense view of the question, the physiological construction and anatomical arrangement of the alimentary canal of a child, prove that it requires animal food. Upon this point, Burdach in his 'Physiology,' and West in his 'Diseases of Children,' speak graphically. It is remarkable that suction is the only faculty for the prehension of food which the child possesses on birth, and even this is soon lost if not practised. The jaws are not so constructed as to permit active movements, nor the gums to bear pressure. The hard palate is, moreover, but little developed; albeit, the cavity of the mouth is sufficiently wide. There is, moreover, no saliva secreted for the first two months, so that no species of preparatory change can take place in it, as in the conversion of starchy matters into sugar, through the agency of this fluid (saliva). It is, therefore, merely an organ of transmission and suction. The lips are large, and the tongue and pharynx, uvula, soft palate, well developed to secure these ends.* The stomach in infants is a small tube-shaped membrane, dilated in the centre, one extremity ending in the œsophagus, and the other in the pylorus, resembling in this character that found in carnivora through life. In position, also, it lies more parallel to the trunk; the large and small curvatures and muscular structures being but very little developed. The liver at birth is unusually large, the pancreas perhaps not more developed than the salivary glands: the in-

* Burdach, p. 434.

testinal tube is much shorter, and the large intestine approaches more nearly in its length to the small. The cæcum (in which, moreover, it is believed a sort of additional digestion occasionally occurs,) is very small. The peristaltic motion is more rapid. All these are evidences that food taken will be kept for a shorter time in the canal, and, therefore, should be in the condition most favorable for digestion.* In none of the mammalia, lastly, is there such a complete absence in the first periods of life, of teeth. In man they appear latest, and are longest in obtaining their total number. Let us compare these appearances with those observed in herbivorous animals. Well-developed salivary glands, compound stomachs, sometimes four in number, muscular gizzards, as in some birds, long intestines, large cæcum, etc., etc., are the exact opposites to what we find in young infants. As the child grows, the changes which are permanent in herbivorous animals make their appearance. The stomach, moreover, assumes a more horizontal position, the vulvulæ conniventes become well developed, the peristaltic motion of the intestines becomes slower; in fact, all the changes calculated to retard the food in its progress, and thus to expose it more completely to the solvent juices for digestion occur, the reverse of what we find in infants, all of which proves indubitably that animal, not vegetable food, is the proper diet for an infant.

If among granivora and herbivora the food essential to their preservation when very young is animal, *à fortiori*, is it the case with the omnivora and carnivora. As man belongs to the omnivorous class, there must, however, be a time when vegetable food may be given. There is no doubt a relation between the period of time occupied in incubation, and the time when an animal is so far developed and grown to partake of herbivorous food without danger. Thus, if a granivorous bird occupy three weeks in incubation, a mammal one month, we should, *à priori*, expect the offspring of the former would be sooner capable of maintaining life independently of its parent than the latter. Again, the same thing would apply to an herbivorous animal provided with a stomach fitted for digestion of vegetables, a compound stomach, as compared to a carnivorous animal, with only a membranous stomach, even though the period of gestation were the same in both. Thus, in the cow and in a woman, gestation has the same duration; but in the one case, the calf, we have the compound stomach, in the child the simple membranous tube, and so the former depends less upon its parent, and attains independent existence and maturity soonest. The best test, however, of capability of independent life in man is the dental apparatus. The appearance of the teeth is our only guide that a child is maturing rapidly, or the reverse in that condition when vegetable food may be safely administered.

Now it would appear that the eighth month is about the period that vegetable food may be borne. The teeth which appear are not of value as capable of mastication, but indices simply of sufficient development in the organs of digestion, which progresses, *pari passu*, and that the salivary and pancreatic glands, the glands of the membranous stomach, are capable of doing duty. The eighth month is, therefore, about the earliest period when a change of food may be given, and, consequently,

* West, pp. 402, 403.

weaning may be tried, if necessary. But even in this case the most easily-digestible vegetable aliment only should be administered, and then it is best to continue also, in great measure, the animal milks.

As I have elsewhere said, animal food is, as it were, the essence of vegetable food, but far more digestible. But there is another peculiarity possessed by animal food. Liebig has shown that the blood in the body is preserved alkaline in carnivorous animals through the agency of the subphosphate of soda; whereas, in the case of herbivorous animals the salt then maintaining the alkalinity of the blood is the subcarbonate of soda. This last result, however, only applies in the case where the food consists exclusively of the lowest grains, roots, green vegetables, and fruits, the ashes of which contain carbonates; because if lentils and the higher cerealia, as wheat, oats, etc., be employed, then, as their salts are nearly the same as the salts of blood, the subphosphate of soda is the salt found in the blood. But more than this; in meat, and the higher cerealia, not only have we a large quantity of mineral ingredient, but we have also a large quantity of plastic or nitrogenous element. The hydrocarbonaceous, calorifiant, or combustible element contained is also in fair proportion, so that many of them may then be safely used. Still there is a very great disparity between these vegetable substances among themselves, as compared with animal compounds. In order to make this clear, I have annexed the following table, compiled from Liebig and R. D. Thompson, in which the amount of nitrogenous or plastic matter being expressed by ten in all cases, the relative amount of combustible or respiratory material is given for the purposes of comparison.

Proportion of ten plastic to the following quantities of respiratory matters in the following articles of consumption:

Veal	1	Rye flour	57
Hare	2	Barley	57
Beef	17	Maize	70
Lentils	21	Potatoes, white	86
Beans	22	East Indian rice	100
Peas	23	Dry Sweedish turnips	110
Fat mutton	27	Potatoes, blue	115
Cow's milk	30	Rice	123
Linseed	30	Arrowroot	260
Fat pork	30	Tapioca	260
Human milk	40	Sago	260
Wheat flour	46	Buckwheat flour	130
Oatmeal	50	Wheat starch	400

The respiratory ingredient in these vegetable substances with large figures being chiefly starch (such as, if digested at all, becomes converted into sugar), would lead, as shown by Majendie's experiments, to the development of scrofula, from a deficiency of plastic or nutritive ingredient. But from the non-development of saliva at an early period, it is to be feared even this change would not occur. And this seems, often, at least, to be the case. In a paper published on the 'Diet of Infants,' Dr. Stewart, of New York, in speaking of the Parisian hospitals, says, "It is the custom at these and similar in-

stitutions, whenever an infant is sick, to withdraw him altogether from the breast, and to substitute for the milk some farinaceous substance, made fluid by boiling—arrowroot, gum, and rice water, or a thickened preparation of rice, known as ‘crème de riz,’ and other preparations of a similar kind forming the diet of a sick infant. In the reported cases of the Foundling Hospital, and those for the reception of sick children, prescriptions of this nature form a very important part of the treatment, as will be seen by referring to the different treatises in French on the diseases of children.” “The attention of M. Guillot being directed to the changes which the food given to children underwent, and to the excessive mortality among them, he instituted a series of investigations in a number of cases of death, with special reference to the state of the contents of the bowels. He was struck with the uniform similarity—a jelly-like substance being present in the bowels, and in some instances lining both the small and great intestines. This was subjected to the test of the tincture of iodine, which produced an intensely blue colour, thus proving it to be starch.”* This jelly-like substance is sometimes tinged with blood. Its presence, however, in the bowels of a child proves that starch is not digestible, at least in the early periods of life, which is, in fact, what we might have anticipated. In adults it is converted into sugar; but if this change is not effected in the child, in whom two of the principal organs that bring about this change do not act at all, or at least very imperfectly, the presence of starch in the bowels in any excess must be detrimental and injurious. Yet how frequently, even by medical men, is arrowroot ordered in cases of diarrhoea as the exclusive diet!

A favorite substitute, also, for human milk is barley—or, more properly, patent barley. Here, again, is a flour comparatively poor in nitrogenous material. But, besides this, it contains dextrine, a substance which even in the adult is difficult of digestion, and, *à fortiori*, must be so in a little infant. Its starch-corpuscles are less soluble in the gastric juice, the milk is slightly acrid, and somewhat laxative.—(Hassall.) When barley paste is washed, the milky fluid deposits, as well as the starch, a protein matter, supposed to be insoluble casein.

Next in esteem with the public is pap. Now pap is given very early. I have seen it given to a child from birth. It seemed to thrive upon it at first: but in about a month’s time the child, which was enormous for size, sickened, and recovered only after much difficulty. Now, here the popular prejudice in favour of white bread proves often a source of death. To show this distinctly, however, it will be necessary to recur to some of the saline constituents of wheat, as compared with those of milk; the comparative disadvantage of wheat-flour, as given in bread, being the decomposition of the phosphates into insoluble, and therefore useless, salts to the economy, and also to a marked deficiency in chloride of potassium.

The salts of milk are not the least important of its constituents. They are stated in the annexed table for human and cow’s milk:

* Dr. Stewart on ‘Diet of Infants,’ Dublin Journal, 1845, pp. 141, 142.

MEAN OF TWO EXPERIMENTS.

<i>Cow's Milk.</i>		<i>Human Milk.</i>	
Phosphate of lime . . .	2.84	Phosphate of lime . . .	0.706
Phosphate of magnesia . . .	1.06	Carbonate of lime . . .	0.069
Phosphate of peroxyde of iron07		
Chloride of potassium . . .	1.63	Other salts . . .	0.053
Chloride of sodium29		0.098
Soda43	Sulphate of soda . . .	0.074
	<hr/>		<hr/>
	1000		1000

Schwartz, in his 'Journal' (vol. viii, p. 270), mentions as contained in 100 parts of human milk the following enumerated salts: Soda, resulting from the decomposition of lactate of soda, 0.03; chloride of potassium, 0.07; phosphate of soda, 0.04; phosphate of lime, 0.25; phosphate of magnesia, 0.05; phosphate of iron, 0.001.

I pass on to speak particularly of the phosphate of lime. This salt, especially when combined with carbonate of lime, is most important in the process of alimentation. It is upon their combined agency that the solidity of the skeleton depends. Moreover, the peculiar property of phosphate of lime in enabling blood to take up more carbonic acid, is not one of the least interesting of its uses. Its administration, whether in a separate form or in aliment to a growing animal, is thus peculiarly indicated. Deformity of every kind in the skeleton may depend on an insufficient quantity of this salt; for it should be remarked, that not only is it useful because it is itself appropriated into the system, but also because by its peculiar influence on carbonic acid it increases the quantity of carbonate of lime held in solution in the blood, and facilitates in this way its deposition in the bones. Chalk, or carbonate of lime, is insoluble in distilled water; but in proportion as this becomes saturated with carbonic acid, so it takes up a larger quantity of this chalk—a property never to be lost sight of, when it is wished to strengthen a growing child. Again, "The phosphate of soda has an alkaline taste and reaction like the carbonate, and its solution in the presence of free carbonic acid takes up as much of that acid as carbonate of soda does; and, like it, only more easily, gives it off by agitation in vacuo, or by evaporation, without losing its power of again absorbing carbonic acid. Hence it follows that the change of acid combined with alkali by phosphoric acid has no pernicious influence, and *vice versâ*, because it gives rise to no alteration in the essential properties of the blood. The processes of sanguinification, of the production of heat and secretion, are carried on alike under the influence of the predominating alkali," as before stated.*

But phosphate of soda seems to possess another useful property in the economy. The fatty acids, stearic and margaric, are converted into emulsions in the chyle through its agency, so as to allow of their easy assimilation in the system. This peculiar property, discovered by Dr. Marcet, and lately exemplified by Dr. Thudichum before the

* 'Liebig's Letters.'

Medical Society, is of immense importance in the explanation of the digestion of fatty matters, and is another reason for supplying food rich in phosphoric acid and soda, which is especially the case with animal aliments, to growing and weakly children; fat, it being well known, being the nucleus around which albuminous matters are deposited.

Of phosphoric acid in particular as an acid, and viewed in its regard to alimentation, there are several very interesting points of view. The blood is alkaline, and, as opposed to this, flesh is acid, this acidity being due to phosphoric acid. In vegetables, also, the excess is on the side of the alkali, except in the case of the more nutritious kinds of grain, which are rich in phosphoric acid.

There is one peculiarity in the solid portions of animal food, flesh, and muscle especially. These contain excess of phosphoric acid; but in muscle, and in soup made from muscle, we have also excess of chloride of potassium in lieu of chloride of sodium. Now there is considerable analogy in this respect in milk which contains an excess of chloride of potassium, although it also contains some chloride of sodium. The following table will show this :

Composition of Ashes of Flesh. (Keller.)		When boiled there enter into the soup.	Composition of Ashes of Milk. (Com.)	
Phosphoric acid . . .	36·60	26·24	Phosphate of lime . . .	50·7
Potash	40·20	35·42	Phosphate of magnesia . .	9·5
Earth and oxide of iron .	5·69	3·15	Phosphate peroxide of iron	1·0
Sulphuric acid	2·95	4·95	Chloride of sodium . . .	5·0
Chloride of potassium . .	14·81	14·81	Chloride of potassium . .	27·1
(' Liebig's Letters,' p. 428.)			Soda	6·7
		100		100

No doubt its large excess in the milk answers many of the purposes of the chloride of sodium in the economy. Chloride of potassium enjoys the peculiar property, however, in common with carbonic acid, of dissolving carbonate of lime or chalk. Its use, therefore, to the infant, for holding in solution in the blood this chalk for the purposes of the skeleton, and supplying to the muscular system a salt essential to that structure, must appear at once obvious.

To return, however, to pap—and the first remark applies to most of the grains, if we except the pea and bean tribes, which are all deficient in the same way. There is no chloride of potassium in wheat, etc., and, necessarily, in bread. But more than this, the phosphoric acid is completely neutralized in its effects. Englishmen like to use white bread, which, independently of containing less nutritive matter than brown bread, as I have fully shown elsewhere, contains alum. This adulteration is known to make inferior flour, and of a bad colour, white, and in appearance equal to flour of superior quality; and, secondly, it enables flour to retain a larger quantity of water, by which means the loaf is made to weigh heavier.—(Hassall.) The bread is also less liable to crumble as it gets stale. Accum, quoted by Hassall, states the smallest quantity of alum that can be employed to produce this white

appearance is 4 ounces to a sack of 240 lbs. Dr. P. Markham states 8 ounces to be the usual quantity employed, and Mitchell found in the 4 lb. loaves he examined the amount of alum varied from $34\frac{1}{2}$ to 116 grains in each. 114 grains would amount to 20 ounces to the sack. —(Hassall.) In 28 samples of bread in London examined by Dr. Hassall in all was alum found, in smaller or larger quantities. The injurious effects of alum cannot be too strongly urged. Alum forms with phosphoric acid, as Liebig has shown, an insoluble salt, thus preventing the phosphoric acid from being appropriated to the economy. The blood thus becomes incapable of performing its duty, and hence the child deteriorates, and in the end will die. And herein is the explanation of that frightful amount of disease in pap-fed babies. The phosphoric acid, so essential to them, is lost altogether. The brain and nervous system, the bones are arrested in their development; and hence also the explanation of the great comparative success in bringing up children by hand in the country on home-baked bread, which contains no alum, and which, although of darker colour, provides phosphoric acid in an assimilable state to the child. But there is another way in which pap proves injurious. It is, perhaps, more often than is recognised, the cause of death. It has long been known that bread and milk, if given to canaries in any quantity, swells in their stomachs, and thus, pressing against the heart, impedes its action, and is often a cause of death. The same result sometimes occurs in the infant. In a paper published in the 'Association Journal' for February, I have enumerated several fatal cases in which the coroner's verdict assigned over-feeding with pap as the cause of death.

Another fraud extensively practised in London is the large admixture of rice-flour in bread. This, I believe, is not generally known; its great whiteness, its great power of absorbing water, are properties peculiarly well known to bakers, and not only ordinary bakers, but many of our hypocritical workhouse-poor feeders. I have been informed by a wholesale corn and flour merchant, that there is a species of rice-flour which is expressly kept for the purpose of adulterating bread, and which is largely employed by our London bakers. In this way the nutritive power of the bread is considerably diminished, although the calorifiant power is increased, the proportion of the former to the latter being, instead of 1 to 7, as it ought to be in wheat flour, increased to 1 in 10 or 11, producing precisely the same results in the human frame as those which follow the employment of a diet too exclusively saccharine, viz. scrofula, atrophy, and all its dependences.

Among the vegetable substances, that which comes closest to milk in its composition is, without doubt, lentil powder, or, as it is called for the purposes of obtaining a better sale, Revalenta Arabica, containing both phosphoric acid in abundance and chloride of potassium; it also includes casein, the same principle which is found in milk in its constituent parts. Moreover, its nutritive matter is to its calorifiant matter in the proportion of 1 to $2\frac{1}{2}$, milk being in that of 1 to 2. No wonder, therefore, that under its influence many children affected with atrophy and marked debility have completely recovered. I have given it with the very greatest advantage in such cases, and, so far as I may judge from my own experience, I should conclude that practice fully carries

out what theory, from a knowledge of its composition, would have led us to anticipate. Lentils have also a slightly laxative effect, and, therefore, in many instances, where the child is of a constipated habit, they are to be recommended. Peas and beans in this respect resemble lentils; the former, however, is objectionable, on the ground that it produces much flatulency. The latter is not generally obtainable; still the bakers take advantage of this fact in regard to the beans, and usually, where wheat by partial germination has lost some of its nitrogenous aliment, or where the flour used is poor in quality, they add a proportionate quantity of white bean flour, to restore it to its proper nutritive value.

The only advantage which another popular ingredient seems to have (I allude to what is called baked flour) is that it contains a smaller quantity of water, which has been expelled during the heating process, and in this respect it comes to resemble more closely, because more concentrated, an animal compound. Moreover, from its greater capacity to absorb moisture, it is somewhat more astringent, and less likely to produce diarrhoea, which, indeed, it often checks; but the absence of chloride of potassium and fatty matters in it, both so essential in growth and all development, is, I think, a great objection to it. Indian corn flour, which contains much oily matter, is preferable to it for this last reason. Hence, if given they should, to supply fat and chloride of potassium, be mixed with milk.

The conclusions to which the present paper leads me are—

1. The analogy of comparative anatomy of warm-blooded animals, and the special anatomy of a child's alimentary canal, indicate that its food should be animal.

2. The child should not be weaned, if it can be avoided, before the eighth month. At this period it may be allowable to give vegetable food, but animal is better.

3. The vegetable aliment selected should contain chloride of potassium and phosphoric acid among its mineral ingredients, and a due proportion of plastic as compared with calorifiant matters; excess of starch being very difficult of digestion.

4. If pap be given, it should be made with milk, so as to include fat and chloride of potassium in the compound, and not given in large quantities; above all, it should not be made with white town-made bread, which contains alum, and is nothing better than a slow poison.

ART. 4.—*Regulation Diet of the Paris Hospitals.*

By Dr. GEORGE SUCKLEY, late Assistant-Surgeon in the U.S. Army.

(*New York Journ. of Med.*, July, 1858.)

The following is the diet system of the Parisian hospitals: The aliments are divided into bouillons, potages, soupes (au pain), aliments solides (solid aliments), and boissons (or nutritious drinks)—namely, wine and milk.

The diet of a healthy man in his natural state is assumed at about 1350 grammes of solid food (about 42 ounces). This, *at first*, when the general diet of the hospital patients became systematized, was called *one*

portion; patients confined to less being ordered one quarter, one half, &c., of a portion.

It was found, however, that the sick were very much dissatisfied at eating *parts* of a portion. In consequence, the full portion was divided into four parts, each called *portions*; and it was then found that the patients, who grumbled most lustily at being restricted to three quarters of the old portion, were perfectly content with *three portions* of the new standard; although in reality the allowances were equal in weight.

There have been, however, several alterations in details, and the single portion, as now used, although approximately in direct proportion to the old full portion, is still slightly varied. This is also the case with the others.

To give a correct notion of the shifting value of the different portions as now used, it is necessary to go into the following details. But I will premise by saying, that a patient confined to what is strictly called *low diet* is only allowed *broth* without bread, farina, or vegetables. The fixed daily amount of this article is a quarter of a litre given twice, making in all a half litre per diem.*

A patient allowed a little higher diet has two broths and two soups a day. The soups contain a little bread or farina, rice, &c. One *soup* contains a small trifle more than a quarter of a litre.

When one portion is prescribed it contains: Soup, twice a day, a quarter litre each time; bread, a quarter kilogramme (about half a pound); meat, six decagrammes (about two ounces); wine, three portions, each containing about two and a half ounces. This wine is light, red wine, coming from the middle of France. Milk, three fifths of a litre daily, boiled. Note.—The wine and milk may be exchanged for each other, at the patient's option.

Meat varies in quantity somewhat according to the number of portions, but not exactly. When one portion is ordered the patient is supposed to be weak, and therefore meat of a better quality is ordered, such as roast beef, or fowl. The same quality is allowed when two portions of diet are ordered, but *two portions of diet* do not contain double the quantity of meat that *one* does; although a little more than this latter, being seven and a half decagrammes (about two and a half ounces). Three portions of meat represent twelve decagrammes (about four ounces). Four portions, eighteen decagrammes (about six ounces); but the meat of the third and fourth portion allowance is *boiled beef*. The usual amount of vegetables for one portion of diet is fifteen decagrammes (say five ounces); but of the coarser kinds, as potatoes and cabbage, double weight is given.

With all the portions there is a little boiled fruit or confiture allowed.

Fish is given twice a week in lieu of meat, and a little more by weight allowed than of meat. The regular proportion is kept up with everything but meat and milk. The milk of the hospitals is contracted for by the general administration, and is supplied every day fresh and pure.

It is the duty of the chief apothecary of each hospital to analyse this milk daily, and also to examine it with the polarimeter (sugar deviating the rays of polarized light).

Four portions of diet complete contain: Soup, morning and evening;

* The litre is equal to gall. 0.22.

meat, six ounces a day; bread, a pound; wine, from twelve to fifteen ounces.

No milk (except by replacement); vegetables, twenty ounces, and more when potatoes, &c., are given.

Extras.—Chop, beefsteak, Bordeaux wine, eggs, chicken, &c., are allowed only upon a *special written prescription* of the attending physician. Eggs, however, are sometimes given in replacement of meat; say one egg to replace two ounces of meat. The rule of the hospitals is to give *meat* itself at least four times a week.

Four portions are not often allowed in the hospitals of the city; as a patient eating them is supposed to be convalescent, and able to go to the hospital for convalescents at Vincennes.

A surgeon occasionally allows a patient five portions, but this is done very rarely.

"The foregoing diet," says Dr. Suckley, "seems to be excellent and liberal for the usual run of *medical* patients, but it seems far too little to support those who are being rapidly weakened by excessive and long-continued suppuration.

"We misguided Anglo-Saxons think that patients in this condition crave and often require a much more considerable portion of food than men in a healthy state. This is to compensate for the drain of the suppuration. The stomach comes to the rescue of the poor suffering system. Should we not, they ask, put into this stomach what it demands? The building-up treatment of the English, or of our surgeons at home, casts far into the shade even the apparent prodigious allowance of five portions, by the French surgeons."

ART. 5.—*Observations on excess of Diet as a cause of Disease, and on its connexion with a hitherto unrecognised hypertrophic condition of the Lungs.* By Dr. RENNIE, R.A., Surgeon to the Convict Establishment in Western Australia.

(*Proc. of Med.-Chir. Soc.*, June 8, 1858.)

In this paper the author states that he was first led to an investigation of this subject on entering upon his duties as surgeon to the Convict Establishment at Freemantle, in June, 1853, by noticing a remarkable prevalence of cutaneous eruptions and other affections which were entirely confined to the convicts, the explanation of which he discovered in their excessive diet, which consisted, amongst other articles, of 27 ounces of bread, 16 ounces of fresh meat, daily, some of the men being allowed even more, and each prisoner having also a daily allowance of $\frac{1}{4}$ ounce of tobacco. It was noticed, that the reconvicted prisoners, who had less diet and no tobacco, suffered far less than the general body of the prisoners. It appears that defective means of restraint was, in a great measure, the cause of this great allowance of food being continued by the convict authorities, on the ground that it facilitated their control by moral force. In December of the same year, ophthalmia and dysentery began to appear, and these, too, only among the prisoners, for the most part taking the place of the cutaneous diseases, and leading to the inference that they were mere local varieties of a general constitutional disorder, and, in most instances, were cases of pure metastasis of the

cutaneous eruptions. The troops and population generally were entirely free from these complaints, so that there could be no reasonable doubt that they originated in the diet being in excess of the systemic demand; and it must be borne in mind that from October to May the temperature in Western Australia is almost tropical. Under these circumstances, he (Dr. Rennie) made strong remonstrances on the subject; but the convict authorities being unable to comprehend the scientific principles involved in the adaptation of diet to climate, and taking as a precedent the very large diet given in the English prisons, as well as being strongly prepossessed against any reduction of the scale on disciplinary grounds, received his suggestions unfavorably. The result was, that during the year 1854, with a daily average of 706 prisoners, 2921 cases of sickness occurred, of which 1058 were treated in the hospital. Another urgent appeal was then made by Dr. Rennie, in which all the scientific bearings of the subject were fully discussed; and though the convict authorities still continued to oppose his views, Governor Fitzgerald ordered a medical board for the consideration of the question. This board, of which the principal medical officer was president, completely bore out the correctness of Dr. Rennie's objections to the diet, and recommended a gross weekly reduction of 136 ounces. The reduction scale came into force on the 1st of June, 1855, and the results proved increasingly satisfactory, as there was abundant evidence by statistical tables to show. Dr. Rennie having arrived at the conclusion that the immediate cause of disease amongst the convicts in this colony was an excess of food operating on men with impaired constitutions, and consequently with impaired powers of digestion, devoted himself to an inquiry into the physical cause, which he felt satisfied was developed previous to their arrival in that colony, and succeeded in identifying it with a hypertrophied condition of the pulmonary parenchyma, and a remarkably adherent state of the abdominal and thoracic viscera, generally unaccompanied by tubercular deposition, or any indications whatever of previous inflammatory action, and leading him to believe it to be a pure form of hypernutrition. The proofs and evidences which led to this conclusion were entered into at great length, as also the appearances, which, with few exceptions, were found in upwards of eighty post-mortem examinations. The portions of lung found most hypertrophied were the upper part of the right lung, and the lower portions behind. The lung-tissue in these parts resembled the appearance described by Laennec as carnification, and at first the condition was viewed as one of chronic pneumonia, but this was disproved by the fact that it was present in nearly every case, and also that, almost without exception, the men so affected never had been, and were not at the time, affected with any symptom of chest-disease. Dr. Rennie, therefore, attributed these abnormal symptoms and appearances to the prolonged operation of a diet too bulky and nutritious for the peculiar circumstances (hot cells and sedentary occupations) under which the convicts are placed during the earlier portions of their imprisonment in England. As the question has naturally presented itself as to whether these extraordinary appearances were developed before or after the prisoners landed in western Australia, and with the view of ascertaining the condition of the respiratory organs, Dr. Rennie availed himself of the opportunity of examining upwards of fifty

men immediately on their coming to shore, and in almost every case found a marked difference between the capacity of the two lungs for containing air, the functions of the right one being almost invariably most defective. In most cases there was, in addition, a general absence of healthy respiration, and, curiously, the more so in those men who had previously been totally free from chest-diseases. All this, Dr. Rennie thinks, strikes at the root of the whole dietetic system pursued during the earlier period of their confinement in England; the state of these convicts with respect to sickness contrasting strongly with that of the inmates of the military prison, who have a simple, wholesome, and yet ample diet, and plenty of exercise in the open air; whereas the convicts, during the first twelve months of their imprisonment, are shut up like hot-house plants in a warm cell, employed at a sedentary occupation, and placed on a diet double that allowed to the military prisoners.

ART. 6.—*On the Death-rate of London.* By the REGISTRAR-GENERAL.

(*Weekly Return of Registrar-General, June 27, 1858.*)

The following extract requires no comment of any kind.

"Man was made to live a definite time and to experience an average rate of mortality. But the natural lifetime has not been revealed to us, and the circumstances of no city are such as to give us an opportunity of determining the average mortality of a people living under the most favorable circumstances. We cannot, like the ancient writers, refer to a model republic: we cannot point to a single town in England on the slopes of some of her hills, looking southward over fertile fields or distant seas; bathed in a pure atmosphere; supplied with 'a river of water of life, clear as crystal;' with no impurities resting in its houses or streets for a single day; occupied by a people fed on fruits, grain, meat from healthy places, and leading an active, good, intellectual life. No such city has ever been projected, and is certainly not shadowed out by the watering-places of our own, and still less of other countries.

"Hence, the only standard to which we can resort is derived from the least unhealthy districts of England. The mean lifetime of the people in those districts is forty-nine years; and the mean annual rate of mortality would be 20 in 1000, were it not that the increasing population gives them an undue proportion of young and middle-aged people, by which the proportional number of deaths is reduced to 17 in 1000.

"To apply the standard to London. The population consists now of about 2,721,000 persons; they are of all ages; but upon comparing them with the comparatively healthy districts the *proportion* of young children under five years of age is the same; before the age of 15 is attained the London children are greatly reduced in number by untimely deaths; at 15 to 25 immigrants restore the lost numbers, and from the same source the men and women of the ages from 25 to 45 grow into a great excess; at the ages of 45 to 55 the proportions are the same; after the age of 55 the excessive mortality in London speedily reduces the numbers: the old people, who naturally experience everywhere a high rate of mortality, are not in due proportion in the population of London. By applying the ascertained rates of mortality in the sixty-three comparatively healthy districts, it is found that the annual deaths—if the

chances of life were the same in London—would be 41,668 on the above population, or at the rate of 15·32 in 1000 annually.

“The weekly deaths in London on the above population in such a state of health as is actually experienced in those districts would be 799 on an average.

“The actual rate of mortality in London during the last ten years exceeded 24 (it was exactly 24·46) in 1000, which implies 1275 weekly deaths, or 476 above the healthy average.

“In the last week 1092 persons died in London, or 293 persons in excess of the healthy average. That 293 persons died unnatural deaths during the week is the finding of this great inquest.

“What were the causes of these unnatural deaths? The people of London live as well as the people of the sixty-three districts; and they now suffer nothing from cold. Many drink spirits to excess. Too many sleep in the same rooms; and, as in our barracks, this destroys large numbers. Crowding in ball-rooms, in theatres, in churches and chapels to hear popular preachers, where no adequate ventilation is carried on, propagates zymotic diseases. Impure water is the cause of several deaths; but the companies have of late years supplied water infinitely superior in quality to the water which they drew previously from the parts of the Thames, now admitted by everybody, even their retained chemists, to be offensive. The impurity of the air was unquestionably the cause of a large number of the 293 deaths. This impurity is most noxious in the houses where the people sleep. The cesspools are still numerous; half a million water-closets and sinks discharge large quantities of impure air into the 353,326 inhabited houses. This incommodity is lessened in London by the system of drains, which, however, are badly constructed, and emit their volatile impurities under the faces of the people. The drains again pour their contents into the Thames; from which, in its course through London in ordinary times, more than four million gallons of water are evaporated daily, carrying with the vapour, and diffusing all over the town, impurities which are breathed by the whole population.”

ART. 7.—*On the production of Pulmonary Consumption in persons who work in a close and confined atmosphere.* By Dr. GUY, Physician to King's College Hospital.

(*Dr. Beale's Archiv. of Med.*, No. II, 1858.)

The ‘Report on the Sanitary Condition of the Army’ recently published proves that our soldiers, but especially the infantry of the line and foot guards, are subject to a very high mortality, a great part of which is attributed to pulmonary consumption. It is assumed in the report that this excess of mortality from consumption is traceable, at least in part, to the narrow space allotted to the soldier in the barrack and guard-rooms: but as no proof of the dependence of pulmonary consumption on this cause is given in the report, it may be useful to republish, from my evidence laid before the Health of Towns Commission in 1844, the following table based upon measurements of the offices of letter-press printers, and the number of compositors working in them, together with

the answers given to certain simple questions addressed to the men themselves.

	Number per cent. subject to	
	Spitting of Blood.	Catarrh.
104 men having less than 500 cubic feet of air to breathe }	12.50	12.50
115 men having from 500 to 600 cubic feet of air to breathe }	4.35	3.48
101 men having more than 600 cubic feet of air to breathe }	3.96	1.98

It is scarcely necessary to add that the number of compositors who answered the question, Had they ever spit blood? in the affirmative, would correspond very closely to the number actually suffering under consumption; just as the number stating that they were subject to colds would afford a good indication of the number in the three classes who were predisposed by the same close and confined atmosphere to suffer by exposure to the common causes of diseases of the chest.

ART. 8.—*Effects of Arsenite of Copper upon paper-stainers.*

By Dr. GUY, Physician to King's College Hospital.

(*Dr. Beale's Archiv. of Med.*, No. II, 1858.)

In the correspondence which has lately taken place in the medical journals and in the newspapers, on the effect produced upon the health of persons living in rooms covered with arsenite of copper paper, nothing has as yet been said on the more important question of the effect produced upon the health of the men engaged in the manufacture of the paper itself.

The colouring matter is mixed with size, and kept in a proper state for use by the heat of warm water. It is laid on to the paper with brushes, and suspended in the warm room to dry. When a bright green colour is to be produced, the arsenite of copper is used alone; but the lighter tints of green, down to the very palest, are obtained by mixing the arsenite with oxide of zinc or porcelain clay, or (for paper-hangings) with whitening. The colouring matter in the act of being laid upon the paper, stains the hands of the workman, and collects under the nails, and being absorbed into the system, produces the effects now to be described. After working one day with the emerald green, a papular rash makes its appearance at the junction of the nostrils and upper lip, then successively on the chin and back of the head, and, after working two days, on the eyelids. The rash also appears at the bends of the elbows.

These appearances are followed by irritation of the scrotum, which terminates in the formation of superficial round ulcers, looking as if they were cut out by a punch; they are about the size of a split pea. The rash, which is originally of a papular form, goes on in parts to pustulation. The rash on the skin lasts about five days; the ulcers of the scrotum are more obstinate. The rash is very painful, especially in the evening. Lithographic printers use the emerald green, and the boys who spread the powder suffer more than even the paper-stainers, especially from

itching of the scrotum. In some instances the scrotum is first affected; and in some other cases it is the only part that suffers.

Dr. Guy has had, as patients, four men, employed in the manufacture of green paper for book-covers and similar purposes (not for paper-hangings), and he has taken this account from their statements; as well as from personal inspection and inquiry at the manufactory where they are employed. The symptoms present in these patients, in addition to those which have been just stated, were inflammation of the conjunctivæ, not accompanied by any tenderness in the epigastrium. Of two patients, in whom the number of the pulse was counted in the erect posture, one had a pulse of 120, the other a pulse of 80. One of the patients, at the time when he applied at the hospital, had three or four open sores on the scrotum, one as large as a fourpenny-piece and one as large as a sixpence; he had also the rash behind the ears, and at the bend of the elbow, and he attributed the loss of a toe-nail and the expected loss of the nail of the right thumb to the same cause. Another workman stated that he was in the habit of working with arsenite of copper for one or two days at a time; that it made his eyes smart and his nose and chin sore. The men stated that they were not able to continue at work for more than about three days at a time, that they use no precautions, but were in the habit of taking a dose of castor oil occasionally.

ART. 9.—*On the maladies of persons occupied in preparing Sulphate of Quinine.* By M. A. CHEVALIER.

(Gaz. Méd. de Paris, May 22, 1858.)

In a communication to the Academy of Sciences in Paris, M. Chevalier states that the workmen concerned in the manufacture of sulphate of quinine are subject to a cutaneous eruption, and also to a peculiar fever. The cutaneous eruption in some instances is very severe; not unfrequently it obliges the workman to remain away from his work for a fortnight, a month, or even longer still; sometimes it makes it necessary for him to seek some other employment. The fever has not been observed in the manufactories of France, but only in that of M. Zurimer at Frankfort, and there chiefly in those occupied in pounding the bark. This fever, also, may be so severe as to oblige the workman to relinquish his employment. As yet no prophylactic measures are known; and the temperate and the intemperate would seem to be affected equally. No further particulars are given.

ART. 10.—*On the influence of the Woollen Manufacture on Health.*

By J. B. THOMSON, Surgeon to the General Prison, Perth.

(Edin. Med. Journal, June 1858.)

In 1853 Professor Simpson published a very able and ingenious essay, on 'External Oil-inunction,' in which he endeavours to establish the following propositions, viz.:

1. That the operatives in the wool factories are a healthy class, and that the oils among which they work undoubtedly contribute to the promotion of good health.

2. That the oils in the factories pass into the system chiefly by the *skin*, and perhaps by *inhalation* also, thereby improving the constitution.

3. That oils rubbed into the skin, or absorbed by bathing, are important remedies for arresting or averting diseases arising from defective nutrition.

4. That singular exemption from epidemic influence seems to belong to all those operative classes much engaged among oils.

5. That external oil-inunction is a cleanly process, and deserves, at least as an adjuvant, to be actively used for the prevention and treatment of scrofula, consumption, etc.

These propositions appear so valuable, that Mr. Thomson, is anxious to strengthen the proofs regarding them by the following statements and statistical tables.

There is nothing new under the sun. The virtues of oil externally applied to the human body are noticed as early as the dawn of human history. In the Bible, oil is spoken of as applied to the consecration of the Hebrew priesthood, and for other holy uses, as a type of Divine grace and goodness. It is also praised as an article of value, a luxury, in the same category with one of man's chief blessings: "Wine, that maketh the heart glad; and oil, that maketh his face to shine." From the siege of Troy to the fall of Rome, many of the classic poets are found to make reference to oil-inunction. The warrior when he went to the field, the wrestler preparing for the arena, was strengthened by oil-friction and bathing—"non olivum vitat;" and the gods and goddesses anointed with fragrant oils, to grace the festivals and ambrosial feasts of Olympus. Nor have the philosophers been silent upon this. Seneca practised oil-bathing; Pliny says, "The human body receives vigour and strength from every kind of oil." Democritus, when asked upon the subject of health and long life, answered with a common maxim of the day, similar to the Scripture expression already quoted, "Apply *wine within* and *oil without*." And one of our own sages, the brightest in what may be styled our Augustan age—Lord Bacon—says, "*Beyond every agent for prolonging life, I know not any equal to the external application of oil to the human skin, 'ante omnia usum olei vel olivarum vel amygdali dulcis ad cutem.'*" Stronger testimony for any remedial agent cannot be gathered anywhere. And it is curious that the moderns should completely have forgotten a substance so highly sanctioned by antiquity.

With extensive opportunities, during seventeen years, while acting as certifying surgeon to the woollen factories of Menstrie, Alva, Tillinacultry, Dollar, and Glendevon, Mr. Thomson's observation has been directed a good deal to the effects of oil upon the operatives. As far back as 1840, he draws attention to "*The influence of the Woollen Manufactures on Health*;" and with the views set forth in that paper he has had more and more reason to be satisfied; no fact connected with the medical topography of this district is more manifest, than the thriving appearance and great exemption from disease of the woollen factory workers, especially children and young persons. It is quite proverbial among the people, and plain to the workers themselves, that weakly children, in a very few months after entering the factories, exhibit a marked improvement in physical appearance. The testimony

of the certifying surgeons from Galashiels, Hawick, and Alloa, corroborates this statement. In rural districts this is notably the case, and in Glasgow and Aberdeen the contrast betwixt the cotton and wool workers is attested, in favour of the latter, by the factory inspectors and certifying surgeons. In Yorkshire, the better classes frequently send the delicate members of their families to the woollen mills for the benefit of their health.

Admitting the proof to be unquestionable, that the persons employed in the woollen factories are a healthy class, we naturally inquire, whence this salutary effect? And the use of oils being that which peculiarly marks this kind of labour, we are led to conclude, that in some way or other *the bodies of the workers are improved by the oils* among which they are constantly employed. In some of these mills, and at certain oily processes, the young persons appear as if literally dipped in oil, and merit Burns' epithet of the "*creeshie nations*." In proof of the efficacy of the oil, the author hopes to prove, that those engaged in the *more oily* processes of the factory are the most healthy, and increase in weight more than the others.

Table No. I shows some instances of remarkable increase in weight in three months among what are called *piecers* and *feeders*, who are employed in the *oily* departments. Of these two classes the *feeders* are the more *oily*. This table shows the weights of 100 young persons, from 13 to 18 years, both inclusive, with their increase after three months in factory (including clothes, except bonnet, shawl, and shoes). The results are:

Total weights of young persons when first examined,	8515½ lbs.
Do. do. in 3 months after,	9093½ lbs.
Total increase in 3 months,	575 lbs.
Average increase do.	5¾ lbs.

Some very remarkable cases of increased weight in three months appear in this table, as, for example:

No. 19	gained	12½ lbs.
32	„	14½ „
57	„	21½ „
58	„	15½ „
74	„	19 „
20	„	12 „
92	„	17 „
98	„	22 „

Increase upon 8 persons (selected) in 3 months,	} 134 lbs., or nearly 17 lbs. average.
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In no single instance was there any declension in weight; and one pauper patient, No. 85, labouring under phthisis, the author recommended to try the feeding; even he gained 2 lbs.

Table II gives a comparison betwixt the *piecers* (less oily) and *feeders* (more oily), with their increase. The result is:

Sum of increase on 18 feeders,	119 lbs.
Do. „ 18 piecers,	103 „

The difference in favour of the feeders is 16 lbs. This is the smallest amount of difference that the author has seen from any former comparison, and the numbers should be tried on a more extended scale. Not long ago he weighed 20 feeders and 20 piecers, and the result was about 50 lbs. more in the sum of the weights of the feeders than the piecers.

Table III is a further analysis of Table I, with the ages, sex, numbers, average weights and increase in 3 months (excluding minute fractions).

Table IV gives a comparison of Young Persons' Weights, in Cotton and Woollen Factories and not in Factories :

Ages.	In Cotton Factories.		In Woollen Factories.		Not in Factories.	
	Boys.	Girls.	Boys.	Girls.	Boys.	Girls.
13	71	73	79	80½	75	72
14	76	83	81	86	78½	83
15	88	87	96	100	86¼	93
16	97	95	...	99½	110	90
17	104	100	98¼	127	117¾	102
18	105	106	...	134	126	121

The persons referred to in these tables were all taken indiscriminately from large or small factories, and without any selection.

Of Table I, showing the weight of 100 young persons, it is remarked, that the young persons were weighed with their clothes on, after taking off caps, shawls, and shoes, or any article of clothing that could be readily thrown off. M. Quetelet averages the weight of clothes of different ages at one eighteenth of the whole weight in males, and at a twenty-fourth part in females. This table indicates, that after the age of 13 the development of growth, and the average increase in weight, are in favour of young females. Table III, in which the averages of boys and girls are given, shows this difference between the sexes.

Table IV is remarkable, as it shows a very considerable greater weight at all ages in the woollen factory operatives; and this is especially notable in *young women* after puberty. For example :

Table V.—Weight of Girls.

Ages.	In Cotton Factories.	In Woollen Factories.	Not in Factories.
14	83 lbs.	86 lbs.	72 lbs.
15	87	86	83
16	95	99½	90
17	100	127	102
18	106	134	121

The difference here may perhaps be explained by stating, that in this district all boys and girls pass their time in the woollen mills, from the ages of 13 to 17 or 18; and these remarkable weights may be partly owing to the oil-absorption of four or five years' continuance. It is probable also that the subjects weighed from the cotton mills, and those *not* in any factory, were taken from the humbler classes, who were but scantily fed. The weights of the cotton-factory children, and those not in factory, are from tables by Mr. J. W. Cowell and Mr. Horner, got at the Lancashire factories, chiefly in the towns of Manchester and Stockport, from which it certainly does appear that the cotton labour is unfriendly to the bodily development; whereas the opposite seems to be the case with woollen labour.

Being curious to compare these weights with those of Dr. Forbes' tables of the weight of students, read to the Royal Society of Edinburgh, a comparison is subjoined.

Ages.	Weight of Students.	Mean weight of Boys and Girls at Woollen Work.
15	112 lbs.	98 lbs.
16	125·5	99½
17	133·5	113
18	139	134·5

Although the wool-factory young persons excel the cotton operatives, and are singularly healthy and well developed, it could not be expected that they should cope with university students, who are generally fresh from the country, and fully as well-conditioned in all respects as any class of the Scottish population. The weights, as above, by Dr. Forbes, moreover, were taken with the dresses, which no doubt would be heavier than those of the barely-clad factory operatives, and therefore some allowance must be made for the difference. Besides, the author took off any clothes that could be dispensed with, of which precaution there is no similar statement made by the professor.

ART. 11.—*On the dangers attendant on the Jewish rite of Circumcision.*

By Dr. WM. MACKENZIE, of Glasgow.

(*Dublin Med. Press*, June 9, 1858.)

It would appear, that Dr. Levit, a German Jewish physician, on a recent occasion, has refused to have his child circumcised, on the ground that circumcision is a severe injury, dangerous to the life of the infant, and which has been known to prove fatal. The subject has been taken up by Dr. Joseph Hirschfeld, who maintains the Jewish rite of circumcision, as performed on infants, to be "in no manner dangerous, much less fatal." The fact is, however, that the dangerous hæmorrhage which is apt to follow circumcision in infants is a subject to which the

attention both of the profession and the state has been often directed in Germany, and especially in Prussia, and which it is vain to gainsay.

Dr. Spiritus, of Solingen, in a set of 'Surgical Observations,' published in the 4th volume of Gräfe and Walther's Journal (Berlin, 1822, p. 292), states, that undoubted instances had occurred of Jewish infants having died in consequence of circumcision unskilfully performed, and that the attention of the Prussian government had been directed to the subject by the Medical College of Berlin. He mentions, that not only from unskilfulness in the operation, but also from abnormal distribution of the blood-vessels of the prepuce, circumcision might be attended by dangerous consequences, as he had himself witnessed. He was sent for, he says, to visit the new-born infant of a Jewish family, and warned that he must make haste, lest his assistance might be too late, as the child, which had just been circumcised, was bleeding to death. On his arrival, he found the assembled family, the rabbi with the rest, in the greatest distress, as neither he nor a surgeon who had been called, had succeeded in stopping the blood, which was streaming from the part. The infant seemed near death, pale, its face the colour of wax, no pulse, breathing scarcely perceptible, while slight convulsive movements showed sufficiently that no further loss of blood could take place, if life was to be preserved. Besides cold water, various other means had been tried to stem the bleeding, including a black powder, the composition of which Dr. S. did not learn, but which the rabbi carried with him for such accidents, and to which he ascribed the power of stopping even the most violent bleeding. Dr. S. immediately cleaned the wound, and ascertained that the blood was spiriting from many small arterial branches. He covered the wound with a thick layer of colophonium, a remedy which he had heard highly recommended as a styptic by Ackermann in his 'Surgical Lectures,' and which consists of the black substance left from the distillation of resin, reduced to powder. The bleeding stopped immediately, and the child was saved. Had another quarter of an hour elapsed, without Dr. Spiritus's assistance, he had certainly perished. The doctor concludes, by holding that this case might recall the attention of the medical police to the subject. Who knows, says he, how often such serious bleedings occur from circumcision, which, if not immediately fatal, may lay the foundation for future bad health? Oftener, too, than is generally thought, he believes extraordinary distributions to occur of the blood-vessels of the prepuce.

Dr. Goldmann, a Hessian physician practising at Schotten, communicated a paper on 'Hæmorrhage after the Circumcision of Jewish Infants,' in the 13th volume of Gräfe and Walther's Journal (Berlin, 1829, p. 201). He was led to do so, he says, from reflecting on the frequency of irregular distribution of arteries, the occasions thereby caused of violent bleedings in operations, the serious import of circumcision when performed by persons unacquainted with anatomy and surgery, and his own experience of the dangers sometimes attendant on this operation.

Mr. L—, a healthy man, by his wife, an equally healthy woman, had had two, if not three, male children, who had died in consequence of circumcision. Another pregnancy occurring, both parents, disconsolate for the loss of their offspring, and the manner of their death, were hopeful of a daughter, that they might not be left altogether childless.

This hope was disappointed, the mother being again confined of a healthy strong boy. Whether the sight of this child caused more joy or fear to the parents, it was difficult to say. According to the Jewish law, circumcised he must be, and certain death seemed likely to follow.

In the greatest affliction, the father came to Dr. Goldmann, asking his advice, and stating, that he had already engaged another circumcisor, from a distance. Dr. Goldmann advised that the child should not be circumcised on the eighth day, but some time later, when he might be present.

Some fourteen days afterwards, when the artist from a distance had arrived, Dr. Goldmann was called, and after having made ready everything likely to be required in case of hæmorrhagy, the operation was done in his presence. The circumcisor was pretty expert at the business, and the operation was effected, bloodily indeed, but satisfactorily.

Dr. Goldmann laid the infant on a bed placed on a table, in a good light, so that he might see whatever happened. Scarcely had some minutes elapsed, when he found the child swimming in blood, pale, and its extremities cold. The child had just before sneezed, which perhaps set the bleeding agoing. Dr. Goldmann found the blood to come from a small vessel of the prepuce by the side of the penis. He immediately applied cold water to the wound, wrapt the child in warm cloths, and gave him some warm drink. The bleeding was obstinate, but he persevered with the cold application, and the result was successful; the blood was stayed, and the child saved. He was convinced that without his presence the child had certainly bled to death; for all averred that the bleeding in the other children had not been so violent, only from the length of time lost in trying various means, supplied by the circumcisor, and other remedies of their own, a fatal termination had ensued.

About half a year afterwards, when Dr. Goldmann visited the same district as vaccinator, he saw and inoculated this child. He had grown bulky, but his face was pale and tumid, his flesh flabby, and on the whole, he was in a weakly state, probably in consequence of the loss of blood at his circumcision.

In the course of another year, Dr. Goldmann delivered with the forceps the wife of a brother of M. L—, the father of the child whose case has just been related, of a healthy boy. He made the father aware of the danger of circumcision, and, taking into consideration the fate of his brother's children, advised him, not only to put off the operation till the child was older and stronger, but also to let a physician or surgeon be present when it was done.

Whether it was the thought that so many Jewish infants were circumcised without harm, or to save expense, the doctor's advice went for nothing, and the child was circumcised on the eighth day, M. L— being godfather. The operation was finished, and as violent bleeding began, the application of cold water was had recourse to, without calling in a medical man. This means proved a disappointment; the bleeding continued. It was now proposed by some one present to apply horse-dung. The consequence was, that the blood was no longer seen, but the infant became every moment paler and weaker, till at length it became evident that the horse-dung merely absorbed, but did not stop, the blood.

Summoned in great haste, Dr. Goldmann found the child completely blanched, the eyes dull and half shut, lips and nails white, twitching of some of the muscles of the face, no sensible respiration, no pulse, only extremely feeble and intermitting pulsation of the heart; hands and feet and whole body cold, this being the effect both of the weakness and of the exposed state of the child, and the inconsiderate application of ice-cold water.

After an actual mountain of horse-dung, covering the whole region from the mons veneris to the perinæum, and from the one trochanter major to the other, had been removed, a stream of blood was seen running along the raphé. The parts being cleaned with a sponge and warm water, a discharge of bright-red blood, *per saltum*, showed a divided artery near the frænum to be the source of the hemorrhagy.

Dr. Goldmann immediately tied the bleeding vessel, and thus removed all danger of further bleeding, but not of death. The infant lay as if dead, an extremely feeble action of the heart being the only sign of life. The wet cold coverings were immediately removed, and the child wrapt in warm cloths. Dr. Goldmann rubbed him with ammonia and spirit of camphor, while the rabbi administered a few drops of warm wine. In half an hour the natural temperature began to return, with some degree of perceptible respiration. Laid in his cradle, he fell asleep; the wound was examined frequently, and a teaspoonful of strong chicken-broth or warmed wine was given at intervals. Thus was a life saved, of which every one present, and even Dr. Goldmann himself, had almost despaired.

Dr. Goldmann, saw this child again, when he was to be vaccinated, and found him not only pale and flabby, but on the whole, his growth not such as he should have foretold before his circumcision.

The view entertained by Dr. Spiritus and Dr. Goldmann, regarding the dangerous hæmorrhage which occurred in the cases, of which the above is an abridged account, was, that it arose chiefly from anomalous distribution of the arteries of the prepuce. These arteries are in ordinary circumstances derived chiefly from the dorsalis, but partly also from the profunda penis, branches of the internal pudic which anastomose with twigs from the external pudic, a branch of the femoral. No arteries of the body are more apt to present anomalies in their distribution than the pudics, internal and external. The arteries of the prepuce frequently present irregularities in source, course, and distribution; and, as seems to have been the case in one of Dr. Goldmann's patients, they are often much larger on the one side than on the other. That such irregular distributions of the arteries may give rise to extraordinary bleeding after circumcision, seems extremely probable.

In those children who partake of what is termed the hæmorrhagic diathesis (a hereditary constitutional condition of the blood and blood-vessels, rendering bleeding from the smallest wound almost uncontrollable), circumcision must be attended with the utmost danger, if it does not prove always fatal.

On the effects likely to be produced on the nervous system of the infant, by tearing up the lining membrane of the portion of the prepuce which is left with the finger nail, as described by Dr. Hirschfeld, and

on the likelihood of its leading either immediately to tetanus, or in after life to other convulsive attacks, as is hinted by Dr. Goldmann, I think it unnecessary at present to dwell.

On the whole, the doctrine of Dr. Hirschfeld, that the circumcision of infants is "in no manner dangerous, much less fatal," seems sufficiently refuted by the facts recorded by Dr. Spiritus and Dr. Goldmann.

ART 12.—*On a recent Epizootic among Swine in America.*

By Dr. GEORGE SUTTON.

(*North American Medico-Chir. Rev.*, May, 1858.)

For some time past a most serious epizootic has been prevailing among the swine in the states of Illinois, Kentucky, Indiana, Ohio, New York, Massachusetts, Pennsylvania, and Maryland, and this Dr. Sutton has set himself to investigate in a very elaborate and satisfactory manner. At first he thought the disease was a malignant form of measles, but the public gave it the name of cholera—"hog-cholera—" from the diarrhoea and the rapid manner in which in many instances it proved fatal. Swine inoculated with the blood of diseased animals contracted the disease; and other experiments, instituted by Dr. Sutton, showed that the disease, when once produced, was capable of spreading rapidly by infection. Dogs, also, which were chained near the infected sties and fed with the diseased meat, became affected with a similar disease; but there was no evidence that the men who had to do with the swine, either before or after death, suffered in any manner.

"This disease presents a great variety of symptoms. The hog at first appears weak, his head droops, and sometimes in a few hours after these symptoms diarrhoea commences. There is frequently vomiting. In some cases the discharges were serous and clay-coloured, sometimes dark, also bloody, and mucous, resembling those of dysentery. The urine at first was generally small in quantity and high coloured, but as the animal recovered it became abundant and clear; this was one of the symptoms by which the men, who were attending the hogs at the distillery, ascertained that they were recovering. *In a large number of cases the respiratory organs appeared to be principally affected*; there was coughing, wheezing, and difficult respiration. In some instances the animal lost the power of squealing, and the larynx was diseased. There was frequently swelling of the tongue, and bleeding from the nose. In those cases where the respiratory organs were the principal seat of the disease, there was generally no diarrhoea or dysentery. In many instances the disease appeared to be principally confined to the skin; sometimes the nose, the ear, or the side of the head, were very much inflamed; the ear swollen to twice its usual thickness. This inflammation would spread along the skin sometimes over the eye, producing complete blindness. Sometimes one or more legs were inflamed and swollen, and the inflammation also extended along the body. The skin, where it was inflamed, was red and swollen. Some had large sores on their flanks or sides, from three to six inches in diameter. In one instance, at the distillery, the inflammation extended along the fore leg, the foot became ulcerated and sloughed off, and the animal recovered. Some appeared delirious, as if there

was inflammation of the brain. I examined the blood of four hogs which had this disease well marked; they were stuck, and the blood, arterial and venous, was caught in a bowl. It was cupped and presented a well-marked buffy coat. Death took place in from one to ten days after the attack. Sudden changes in the weather, particularly from warm to cold, appeared to increase the fatality of this disease. The average mortality of hogs that were in pastures or fed on slop, was from thirty-three to forty-five per cent., but it was frequently much more fatal if hogs were fed on corn,—in some instances ranging from seventy to eighty out of the hundred, and in some instances even higher.

“I found, on opening the bodies of hogs that had died of this disease, that they all presented evidences of a diffusive form of inflammation. From sixty-seven hogs that I have examined, I found it was not confined to any particular tissue. Sometimes this inflammation was confined to one organ, in other cases it attacked several at the same time. The skin frequently presented patches of inflammation, and often had a purple appearance. In cutting through parts that were the most inflamed, the skin was swollen, and the cellular tissue was infiltrated with serum. Frequently, however, the skin was merely discoloured, without any swelling whatever. The stomach was occasionally distended with food, and the mucous membrane in nearly every instance presented evidence of inflammation, sometimes extending over the whole stomach, at others only in patches; it was generally of a deep red colour, thickened, and frequently softened. Sometimes it was covered with a viscid mucus, in other instances there was an effusion of blood into the stomach. The mucous membrane of the small or large intestines, where there had been diarrhœa or dysentery, presented in all instances evidences of inflammation; in patches it was red, thickened, sometimes softened, and occasionally ulcerated; where there had been dysentery there was generally bloody mucus found in the large intestines. The bladder generally contained urine; sometimes its mucous membrane was inflamed, and in one instance there was an effusion of blood into this organ. In a large number of cases I found evidences of peritoneal inflammation, such as redness of this membrane, effusion of turbid or bloody serum, adhesions between the intestines, and between the intestines and sides of the body. In three instances blood was effused into the peritoneal cavity—in one instance more than a quart; it appeared in this case to come from the liver. The liver was occasionally the seat of this inflammation, not only in its investing membrane, but the parenchyma; sometimes there were abscesses, and in one instance portions of it were gangrenous. The lymphatic glands were generally of a dark red colour, frequently resembling clots of blood. This disease of the lymphatic glands was of common occurrence.

“The lungs were frequently the seat of this inflammation, portions of one or both presenting different appearances, from simple congestion to complete hepatization; sometimes there was ulceration, and frequently there was a turbid, or sero-purulent, or bloody effusion into the pleural cavity; sometimes there were extensive adhesions between the lungs and pleura of the ribs. At first I was inclined to believe this malady to be a form of pleuro-pneumonia, but after I became better

acquainted with it, I found that the inflammation was not uniformly confined to any organ. In a number of instances the mucous membrane of the bronchia was deeply inflamed, and the inflammation extended to the trachea and larynx. In several instances the larynx was inflamed, resembling laryngitis. One animal that had great difficulty in breathing, and could make no noise, I had knocked on the head, and on examination, I found the mucous membrane of the larynx and epiglottis inflamed and swollen; also the tongue was swollen. There was evidence in several instances of pericarditis, which had produced adhesions between the heart and pericardium. The brain, from the difficulty of opening the skull, was examined only in one instance; it was found healthy, although I feel confident it was frequently the seat of the disease.

“From these examinations we see that it is a misnomer to call this malady cholera. It is a contagious inflammatory disease, the inflammation being confined to no particular tissue, sometimes attacking only one, at others, several in the same animal. Evidences of this inflammation were found in the dermoid, the cellular, the serous, the mucous, and glandular tissues. I consider it a diffusive form of inflammation from the manner in which I have witnessed it spread along the skin. In one night I have seen it extend from the eye to the ear—the ear becoming inflamed and swollen. Although we have not been able to show that this is a cholera epizootic, still the facts elicited may be of interest, and remove doubts at some future period. But, then, if this malady does not resemble cholera, does it resemble any of the diseases to which the human system is subject? I think not. Like the specific eruptive diseases, it is highly contagious; the infection has a period of incubation of from twelve to twenty days, and one attack appears to exempt the animal from a second. But in this disease, although petechia and an eruption may appear on the skin, its principal characteristic is a diffusive form of inflammation, which may attack nearly every tissue and spread like an erysipelas. But then, again, it differs from this disease, as it is well known that in erysipelas one attack does not exempt the system from a second; and although erysipelas may be contagious, still it is doubtful whether the period before the eruption shows itself is so uniform as in this disease; and while erysipelas is generally confined to the skin, this inflammation most frequently attacks the lungs and mucous membrane of the alimentary canal. This disease appears to be intermediate between the specific eruptive diseases and erysipelas—partaking of the nature of each, and probably not having its exact resemblance among the diseases to which the human system is subject.”

(B) ACUTE DISEASES.

ART. 13.—*On the theory of Inflammation.* By Mr. JAMES HINTON.

(*Medico-Chir. Rev.*, July, 1858.)

All writers on inflammation have recognised in it processes of two opposite characters and tendencies. Mr. Paget classifies them into those that are productive, and those that are destructive, and the distinction

is broadly obvious. Into the ordinary conception of nutrition itself indeed both these processes enter; it is regarded as including two opposite actions or series of changes—growth and decay. But this oppositeness of action is even more marked in inflammation than in health. In an inflamed part we may see a structure decomposing, not in invisible molecules, or by mere interstitial removal of its elements, but dying in large masses, while all around it the evidences of *vital* action, of the impetus towards growth, are seen in more than ordinary energy. Is there any intimate relation between these opposite actions; may inflammation consist in either alone; or, if both be essential, what is their connexion?

That an increase of both processes, the decay and the vital action, is necessary to constitute inflammation, appears when we consider the distinctive characters of that affection. It differs from mere increased decay, as primary gangrene or atrophy, on the one hand; and from mere increase of vital action—hypertrophy, repair, or development—upon the other. Its peculiar characters involve at once an abnormal increase of destruction and of growth.

If, then, both these changes be essential to inflammation, can there be traced between them any other connexion than that of coexistence? Are they related as cause and effect? Which is the starting-point of the morbid process?

The answer is: they are related as cause and effect; the increased decomposition is the starting-point; the increased vital action is secondary and dependent.

The first proof of this position is found in the nature of the causes by which inflammation is induced. All of these, it has often been remarked, are such as clearly tend to lower the vital power or to produce actual destruction of the parts on which they act. In every case in which the origin of inflammation is distinctly traced, the starting-point is found to be in fact an anti-vital change.

And this practical evidence is reinforced by the most cogent theoretical considerations. Can we represent to our thoughts any clear idea of a primary abnormal increase of the *vital* or formative action that should be inseparable, as inflammation is, from a concurrent increase of decay? And this increased decay not such as attends and is subservient to increased growth, but of so disproportionate an amount as almost always to result in a lessened vitality of the affected part. Is it not a contradiction, that an approximation to death should be the result of an increased life? It is not inquired now how such a primary increase of the formative action should arise, and especially in such circumstances of debility and oppression as most favour inflammation, because that subject will be considered hereafter in tracing the relation between inflammation and adventitious growths; but there is a direct bearing on the question in the fact that inflammation arises in the tumours then first when decay begins in them. It is incompatible with the increased formative action which produces them; it is a constant attendant on their disintegration.

Connecting thus the two series of changes, destructive and formative, as cause and effect, both may be understood. For the increased formative action some cause is demanded, some additional and locally acting

force to which it may be ascribed. This demand is fulfilled by the increased decomposition, which is a known source of force, and which is itself sufficiently accounted for by the tendency of all organized substances to undergo decay. The abnormal decomposition is referable to known and sufficient causes, and itself supplies a cause for the abnormally increased activity of the formative process. For not only is decomposition of the tissues (a change belonging to the class of chemical actions) a recognised source of *force* as such, and thus capable of acting as a stimulus upon the vital activity of adjacent tissues, but it is shown by well-known facts to be immediately concerned in the production of the formative action. Such facts are the liquefaction of certain portions of the embryo as conditions for the development of other portions; the decomposition of the food which forms the first stage of digestion; and especially the immediate dependence of the nutrition of any organ upon its functional activity.

Inflammation indeed stands thus but as an exaggerated instance of this normal relation of decomposition and growth: it is strictly correlated to the ordinary processes of life; an abnormal or excessive functional or decomposing change, producing a similar excess of the reparative action. It may seem strange indeed how so natural an interpretation of the facts should have escaped the sagacity of those observers who have especially noticed the intimate connexion between functional activity and inflammation, and who have remarked that excessive function of an organ leads to or becomes inflammatory action by such continuous gradation that the line cannot be drawn between them.

Inflammation is excessive function, with or without qualitative perversions; *common* in the absence of such perversions, *specific* when they exist. The term "function" is here used to signify that disintegrating change of which the functional activity is an indication. In attributing inflammation to an excess in this respect, nothing is assumed but a known tendency, the chemical affinities, which may always be presumed to act when not prevented by opposing force, or absence of the requisite conditions; and which are therefore necessarily brought into play by all that diminishes the perfection of the vital state. From the operation of these forces all the main phenomena of inflammation may be traced in a consequent series, and no recourse is necessary, as upon the hypothesis of a directly increased vital action, to mysterious, or at least to unknown, powers.

The twofold nature of the processes concerned in inflammation has been one chief source of the difficulty that has invested the subject; these processes not being seen in their true relation, nor recognised as corresponding, in respect to that relation, to the healthy life. Two opposite views have been maintained by different writers, each with great support from observation, yet each failing to supply a theory of the affection that could be accepted as complete, or as applicable to all cases. On the one hand is the theory of "increased action;" on the other, that of "debility," or diminished vital force. Each reposing on one portion of the phenomena, with an insufficient recognition of the other, and embarrassed, therefore, instead of aided, by half of the facts with which it had to deal: each capable of a most plausible demonstra-

tion, yet leaving in the mind a painful consciousness that the problem was not solved, nor the true nature of the disease revealed. The old theory of increased action* demanded as its complement the modern one of debility or diminished action; but the latter, though more philosophical, equally fails to express the whole truth, and had it existed first, would not less certainly have been supplemented and supplanted by the one whose place it has usurped. If inflammation be in all cases merely diminished action, "depression of the vital force," what is the distinction between sthenic and asthenic inflammations? why should stimuli be in some cases useful, in others injurious? Would not the term, "diseases of debility," become then a mere pleonasm, while yet we cannot but feel that it does express an actual and most important distinction between classes of disease which may be both alike inflammatory? And are not greater heat, more rapid circulation, a more vivid sensitiveness, among the indications of a higher life by which the warm-blooded animals are elevated above the cold-blooded? Shall we, to make a theory consistent, permit contradictory interpretations of identical phenomena?

It is not denied that inflammation is, in one sense, always a disease of debility; that is, its starting-point is anti-vital change, it originates in decay; but it includes not less an opposite class of actions, the downward process generates an upward one; decomposition adds intensity to life.

The inflammatory process, then, is an affection primarily due, as all functional processes are, to a disintegrating change which generates a formative process that would not else exist. Thus viewed, inflammation may not only be better understood in itself, but may be brought into definite and intelligible relations with a wide circle of kindred phenomena, mutually giving and receiving light. At first, as to its own nature, it is found to bear a distinct and decisive character. It may be defined. The boundary which separates it alike from health and from other morbid processes is distinct and legible. From health it is distinguished in this, that it is an excess or perversion of the *functional* activity, with its consequences; the decomposition which is normal in function exceeds in inflammation that amount which is compatible with the integrity of the tissues.

And from other local diseases it is clearly marked by these characteristics—of involving a twofold action, and of starting from a decomposition. Tumours may present the twofold action of growth and decay, but the growth in their case has precedence. Hypertrophy presents increased formation only; atrophy, diminished formation, and probably diminished energy of decomposition also. Degeneration, properly so called, if agreeing with inflammation in having increased decomposition for its starting-point, differs from it in the absence of the vital reaction; as also does primary gangrene, though the latter is a cause of inflammation in the surrounding parts. It seems to me that the difficulty, on which so much stress has been laid, of indicating precise lines of demarcation between inflammation and other affections, does not exist if the case be rightly conceived. That various abnormal processes may coexist

* For a most ingenious argument in favour of this view, see a paper by Dr. Cappie, "On the Nature of Inflammation:" 'Edinburgh Medical and Surgical Journal,' No. 81, p. 58.

is true, but there is no necessary confusion among them. Where a local decomposition, carried beyond the bounds of the normal functional activity, has brought in its train an abnormal formative action, in however slight a degree, in whatever condition of the system, or with whatever other morbid processes it may be mixed up, there has been inflammation. The relation of the forms of action concerned in the inflammatory process is well seen in the phenomena attending suppuration. For in the formation of pus-cells there appears to be a true growth; and we may conceive that the force arising from the increased decomposition which has previously been operating upon the solid textures, producing in them the heat and redness and swelling which are characteristic of inflammation, operates after the effusion partly on the effused fluid. So that while the dynamical process remains the same, the "symptoms" begin to subside.

ART. 14.—*On Bleeding in Inflammation.*

By Dr. MARKHAM, Physician to St. Mary's Hospital.

(*Medical Times and Gazette*, July 10, and Aug. 7, 1858.)

The following is a summary of the conclusions arrived at in these papers :

1. There is no proof that venesection has any directly beneficial influence over the course of inflammations, either external or internal. Surgeons never bleed now in external inflammations; and physicians have given up all argument in favour of the proceeding, except in the case of pneumonia, and perhaps also of peritonitis. At all periods of medical history, moreover, it has been especially in pneumonia that the benefits of venesection have been most firmly extolled.

2. But the direct abstraction of blood by leeches, &c., from an inflamed part, during the early stages of the inflammation, modifies its course, and materially reduces the most characteristic phenomena of it, viz., the pain, the heat, the redness, and the swelling; and the abstraction of blood does this, whether the inflammation be traumatic or specific, as we observe, for instance, in the application of leeches to a sprained ankle or to an inflamed joint. There is therefore a marked distinction to be made between venesection and local abstraction of blood.

3. Local abstraction of blood, however, cannot produce the same beneficial results in the case of internal inflammations, except in those instances in which we are thereby able to draw blood directly from the inflamed part. Leeches applied to the thorax cannot draw blood directly from the inflamed lungs; when they appear to be of service in pleuropneumonia, they are so by drawing blood from, and so reducing the inflammation of, the parietal pleura. In endocarditis, again, direct bleeding (over the cardiac region) is useless; in pericarditis it is of great service, because thereby blood can be drawn directly from the inflamed pericardium and pleura.

4. Venesection, where properly used, is of great service, incidentally, in pneumonia. There is a peculiarity in the circumstances attending this inflammation, which causes it to differ from all other internal inflammations; and this peculiarity consists in the mechanical effects—the congestion of blood in the heart—produced by the inflammation. The

bleeding relieves this congestion, it has no directly beneficial influence over the inflammatory process. It serves exactly the same end in pneumonia as it does in the congestions, which result from wounds of the lungs, diseases of the heart, aneurisms, and all those affections which produce great and sudden congestion of the organ. Army-surgeons bleed largely and at once in wounds of the lungs, before the inflammation sets in.

5. The use of venesection, therefore, in pneumonia, is to relieve the cardiac congestion which is produced by the impediment to the circulation of blood through the lungs; it neither arrests nor modifies the inflammation. And the corollary of this is, that venesection is frequently required during the progress of pneumonia, and of many other diseases, for the object indicated.

6. It is not denied, by anything here stated, that local bleeding in the inflammation of internal organs, where there is no direct vascular connexion between the skin and the inflamed organ, may not influence the inflammation by some reflex action conveyed thence from the skin to the vaso-motor nerves of the inflamed organ; but this influence, if it exists, has yet to be demonstrated.

ART. 15.—*On Acute and Chronic Disease.*

By Dr. WELKS, Assistant-Physician to Guy's Hospital.

(*Guy's Hospital Reports*, 3d series, vol. iv, 1858.)

Most of our writers on medicine have hitherto directed their especial attention to acute diseases, making these the types and the forerunners of the numberless changes to which the various organs are liable, looking upon them as the regular forms of maladies in which pathological changes may be best considered and scientific treatment adopted, while the chronic forms are the irregular, or those which result from the acute; and, showing that such opinions are not rare, we lately heard discussed at a medical society the importance of early treating acute disease lest it should become chronic, the idea being that the body in health is liable to sudden attacks of acute disease, and if the latter be not arrested, a chronic affection is the result. Without denying that this may occasionally be correct, our own experience is so different that if the doctrine were reversed we believe it would be far nearer the truth—that disease is mostly chronic, and if not arrested will become acute. It is for the most part an assumption that acute diseases may become chronic, and much more that chronic disease has once been acute, the latter, in fact, in most instances, never having been preceded by an acute affection, the chronic change having been chronic from the commencement, the mistaken notion having arisen from the terms employed having reference to time, a disease at its commencement being considered acute, and after a time chronic; whereas the morbid changes in the two are different *in kind*, being in the latter slow in their commencement and progress, and unaccompanied for a considerable time by symptoms, and thus unlike the former. It has been a habit among us, that when any change has been found in an organ, especially if that be of a fibrous kind, to attribute it to a former inflammation, but this is altogether an assumption, we our-

selves having lately seen a cirrlosed lung with an extremely thickened pleura which had been progressing for five years, and which, having watched from the commencement, we are sure never had any acute origin, and yet it was thought by some that such *must* have been; and the same we believe is true of other chronic diseases. A slow change of this kind does not result, as was formerly supposed, from an organization of lymph, but is rather allied to a growth, and such is necessarily slow or chronic from the commencement; for example, if the beginning of the process known as cirrhose in the liver could be appreciated after the first week of its progress, the change would be chronic, although a hepatitis with suppuration might be called acute if the disease had existed for twice or thrice the time.

We have only to regard the various forms of diseases daily before us to recognise how large a number are chronic, and if we are in the habit of examining our patients after death, we shall also discover how many acute attacks are merely the sequel of long-standing diseases. Among the affections of the brain how many are chronic, or dependent on disease in other parts, as softening or affections of the membranes; and if a person suddenly falls with apoplexy, and is by popular expression said to have been cut off in health, the medical man knows that the vessel which ruptured in the cerebrum had been slowly progressing in its morbid changes for at least many months; and as regards acute inflammation of the brain, a large number of cases are associated with tubercles of slow formation, or result from disease of the bone (as of internal ear), except a few instances in children apparently connected with exanthematous disease, we ourselves never having seen but one case which could be styled idiopathic inflammation of the brain, where no anterior cause was found to account for it. On the other hand, acute meningitis is very often the termination of chronic disease of the brain, as when tumours are present, and thus, as we at first said, the acute follows the chronic, and post-mortem examinations reveal to us acute disease as that which is fatal, and which has resulted from the chronic. As regards the chest, no doubt acute disease of pleura or lung does occur in healthy persons from exposure to cold; but to one such case, we witness twenty of these affections occurring in a lung previously diseased, as in phthisis; or if the organ be healthy, the patient is suffering from some other affection, as morbus Brightii; and thus it is, we think, that no deductions can be made with reference to the seat of pneumonia, or its treatment, from cases taken indiscriminately from the wards of the hospital and from the records of the dead. Without alluding to hypostatic pneumonia (or the pneumonia of the dying), we see at least fifty cases of hepatized lung occurring as a part of other visceral disease to one case of idiopathic pneumonia. Although there are constantly cases of pneumonia treated in the hospital, post-mortem inspection has not revealed a single idiopathic case for more than a year past; and the same may be said of pleurisy, though to a less extent, as we think idiopathic pleurisy is more common; but even here the large majority of pleurisies are occurring in patients suffering from chronic disease. We will add to this bronchitis, and maintain that acute idiopathic bronchitis is a rare disease. In reference to the pericardium, we may remind the reader that acute idiopathic inflammation of this structure is almost unknown. If then, acute

pneumonia and pleurisy occurring in healthy persons constitute the exceptional cases in which such forms of maladies occur, and other acute affections are still more rare, how remarkable it appears for some of our older writers to speak of acute inflammations in healthy persons as typical forms of disease, and those in which the action of remedies is to be studied. In considering the abdomen, nearly all the affections, as witnessed in this country, are chronic; and acute peritonitis, which might be called idiopathic, we have never yet seen. Hepatitis and dysentery are acute in tropical climates, but the changes in the liver, kidney, intestine, &c., as we in our country witness them, are mostly slow. Acute peritonitis in nearly all cases results from some lesion which is chronic in one of the organs which is covered by it, and arises from abscesses bursting into it, or perforations of the hollow organs, or from a local inflammation commencing in an ovary or other part propagating itself throughout the abdomen.

Should acute inflammations arise without a chronic change in this part, there is some constitutional affection implicating the fluids in the body; such as disease of kidney, which is often suddenly fatal by a pericarditis or peritonitis.

The disposition for disease to commence slowly and end rapidly or acutely, is nowhere better seen than in a phthisical lung: towards the apex, where the disease commenced, we see a vomica surrounded by dense tissue, showing the organization and slow process which characterised the commencement of the disease; as we proceed lower down we find deposit of a softer character; still lower down, this resembles the material of gray hepatization, and below this again we often find an acute pneumonia which has carried off the patient.

We do not of course, in thus speaking cursorily on this subject, refer to diseases dependent on some specific cause or poison which may lay hold of any healthy person, as the exanthemata, but we allude rather to local inflammations, which, according to some of the older systematic writers, were regarded as the most common forms of affections, and might occur in any person—they thought that an arachnitis or pericarditis, or a peritonitis, might from such a cause as exposure to cold, be suddenly lit up in a previously healthy person. Now, we are not aware that we have ever witnessed such a case, these acute affections being merely parts of some more general malady affecting the whole system, or attacking an organ previously diseased. This, as before said, is not altogether true of pneumonia, nor of pleurisy, which are constantly occurring from causes above named, although even these affections arise far more frequently in diseased persons. We may state then, in general terms, that disease of the various parts of the body is, as a rule, chronic, and that the acute affections are merely terminations of these, or are set up by them. We are quite aware of the objection, that observation in the wards and in the post-mortem room is of a different kind, and that the very fact of these remarks having been made on the dead, is sufficient to show that such organic changes could not have existed in those who have recovered. A discussion on this point would too greatly prolong this paper, and, therefore, we will merely state, that the proofs of the existence of various diseases during life are slight compared with ocular inspection after death; and therefore if, for example, in every case of

fatal peritonitis a prior cause is found to have produced it, we think the arguments are equally in favour of such cause existing in the cases which recover, as that the inflammation is altogether idiopathic. We should have liked, had space allowed, to have entered upon this subject more in detail, and illustrated it by examples; this we hope to do at a future time; at present merely pointing out to those who have not the opportunities of making necroscopic examinations, the result of our observations.

ART. 16.—*On the changes in the constitution of Fever and Inflammation in Edinburgh during the last forty years.* By PROFESSOR CHRISTISON.

(*Edin. Med. Journal*, July, 1858.)

During the last forty years, Dr. Christison maintains, there has been a change in the character of the symptomatic fever attendant on acute local inflammation, which will account for so great a change in the treatment as the abandonment of bloodletting.

“Unfortunately,” he says, “I am not able to refer to any recorded facts in support of this proposition. But I can conscientiously say, that many years have elapsed since I have made allusion to it in my lectures on ‘Clinical Medicine’ in this university. And I can, with equal confidence, pledge myself to have formed my present opinion long prior to the first germs of the present controversy, and quite irrespectively of controversial bias of any kind.

“The local inflammations, in which the change in the form of the concomitant fever has principally attracted my attention, have been erysipelas, acute rheumatism, pleurisy, and pneumonia. In all of these diseases, during the earlier period between 1817 and 1830, or a little later, it was customary to find the pulse not merely frequent but also either full, hard, and difficult to extinguish by pressure, or small, wiry, and equally hard to compress. This state of the pulse was likewise attended with more heat of the skin and febrile restlessness than I have been accustomed to observe for many years past. In correspondence with these peculiarities, it was not uncommon to observe the blood issue from the vein, opened for the purpose of bloodletting, with great force, and of an unusually florid colour, and occasionally with a certain jerk contemporaneous with the pulse. I have repeatedly seen, at the time now referred to, hospital dressers or other young medical men so much alarmed at these appearances, as to fear that they had cut the subjacent artery, and only relieved upon finding the flow of blood averted by firm pressure of the vein beyond the wound. In no disease was the force of reaction more remarkably demonstrated by these phenomena than in acute rheumatism. I well remember that in my experiments on the changes produced by the air in the blood, performed in 1830 (See ‘*Edinburgh Medical and Surgical Journal*,’ xxxv, 94), I was repeatedly unable to use for my purpose venous blood taken from rheumatic patients, on account of its colour being too little removed from that of arterial blood, to show the change of hue caused by agitation with air.

“The character of the pulse, which I principally miss in the present

time, is its incompressibility, which was a constant object of attention and interest during the fashion of bloodletting, and by which, far more than by the mere frequency or fulness of the pulse, the question of bloodletting and its amount were regulated. Those who have not had occasion to observe this phenomena in acute local inflammation, will understand what is meant, if they have ever attended carefully to the condition of the pulse in some forms of sudden, violent, brief apoplexy, in which, after a short stage of great depression and irregularity, the pulse, gradually rallying, becomes at length almost painfully full and bounding, and so hard to compress that scarcely any force, with the finger's point, will completely extinguish it.

"As inflammations, with this accompaniment, continued to advance in spite of bloodletting; or, when it had been neglected, the condition of the pulse gradually altered, until at length it acquired the same comparatively soft and easily compressible character which is observed to be its ordinary condition at all stages in the present time, as well as for some years past. Now, it is important to remark, that as soon as the pulse put on this altered condition in the advanced stage of inflammation, every medical man practised bloodletting with far greater caution; and I could show, by notes I still possess, of the case of a brother graduate whom I treated at Paris in 1821, in rather remarkable circumstances, that I at least was at that time fully aware of the danger of bloodletting in pneumonia in such a state of the circulation.

"If in those days physicians were aware that the acute local inflammations might, in their course, present characters which contra-indicated bloodletting, it does not appear a very violent assumption, that, if they observed those characters presented at the beginning instead of the advanced stage, they would hesitate to draw blood, and at length, as this character became more and more manifest, abandon it in a great measure, and such I apprehend is the real history of the modern change of men's minds as to the employment of the free evacuation of blood in the acute inflammations.

"An attempt has been made to ascribe the change—1, to an improved acquaintance with the phenomena of pneumonia as one of the leading acute inflammations—the consequence of the use of the stethoscope—and 2, to our acquaintance with the antiphlogistic properties of tartar-emetic. Both arguments are baseless. Acquaintance with the stethoscope will not explain the abandonment of bloodletting in other inflammations besides pneumonia and pleurisy; and yet the remedy was surrendered in all acute inflammations, about the same time. Besides, it is altogether a mistake to assume, that the stethoscope came into familiar use in Edinburgh, and more especially among the medical officers of its infirmary, only about the time when bloodletting began to fall into desuetude. Several practitioners of this city had studied long before that date under Laennec himself; one of these was physician of the infirmary so early as 1827; and others besides him used the stethoscope with as much address and familiarity at that period as any of their successors have done since.

"As little can it be admitted, that professional men of this city have been acquainted with the merits of tartar-emetic as an antiphlogistic remedy only since it began to displace bloodletting in the treatment of

pneumonia. The use of tartar-emetic, as an antiphlogistic in local inflammation in British practice, is of much older date; when I was a young graduate, it was a familiar remedy in the advanced stage of pneumonia, and was used by myself as the acknowledged resource, and with prompt and excellent effect in the case of pneumonia referred to above in 1821; and as for Laennec's plan by large, frequent, contro-stimulant doses, it was employed in the clinical wards by my predecessor, Dr. Duncan, who died in 1832, and likewise by myself and others some years earlier than that date.

"I feel persuaded, therefore, that the more this interesting subject is investigated, and the more that the personal experiences of those who practised medicine between 1817 and 1830 is brought to bear upon it, the more will it appear manifest that a change to a mere asthenic character has gradually taken place since that period in the febrile condition of the circulation attendant alike upon continued fevers, eruptive fevers, and the acute local inflammations; and the more will it appear probable that this change has been the real cause of the change which has taken place in medical opinion and practice as to their treatment."

ART. 17.—*General considerations respecting Fever.*

By Dr. HANDFIELD JONES, Physician to St. Mary's Hospital.

(*Brit. Med. Journal*, Aug. 1858.)

In the following propositions and remarks, an attempt is made to take a general view of the principal phenomena of the febrile state, regarding their pathology from the point of view so well indicated by Parkes and Virchow. The author is far from imagining that we have yet any complete and satisfactory theory of fever; but he cannot think that any one can have any just conception or enlarged view of the pathology of fever, who ignores the capital facts so well elaborated by Bernard.

"1. Fever may result from pure nervous exhaustion. I am acquainted with two medical men who have suffered attacks of fever, to all appearance, in consequence of fatigue in walking. In such cases, it may be presumed that the sympathetic system has become affected, as well as the cerebro-spinal.

"2. The nervous power of the cerebro-spinal system may be extremely depressed without fever being induced.

"3. Paralysis of the vaso-motor (sympathetic) nerves is probably essential to all fevers.

"4. In the majority of fevers, the nervous power of the cerebro-spinal system is greatly debilitated or impaired.

"5. In all varieties of asthenic fever, debility and depression predominate. They may be more marked in some organs than in others, and may be variously combined with degrees of irritation; but still they give its character to the disorder.

"6. All debilitating influences, in their less severe degrees, tend to produce a state of irritability; in their more violent action they cause prostration. The character of irritability is weakness, together with an undue sensitiveness to all stimuli. In such states, there is often a great

display of force ('increased action'); but it is to be remembered that this takes place at the expense of the *radical** forces of the economy, which are drawn upon for the excessive development of the *acting*. Wild delirium and increased energy of circulation can only occur at the expense of the real power of the brain and heart.

" 7. In the sthenic and inflammatory fevers, the heart's action is excited and increased in force, as well as in frequency. The cause of this may be presumed to be, that the blood, being hotter than natural, stimulates the heart unduly. The place of the heart in the vital economy, its highly developed structure, its peculiar power of not knowing fatigue, unlike other muscles, afford sufficient reason why it should be stimulated to increased energy of action, while the arterial muscles are more or less relaxed or paralysed. It has a higher responsive faculty. At the same time, the tissues of the body generally are not so much debilitated as in asthenic fever.

" 8. The causation of pyrexia attending on local inflammations may be viewed as follows. The blood traversing the inflamed part becomes altered in some way (perhaps by having an increase of fibrine generated in itself), so that it comes to contain matter, which may be compared to the miasm of fevers (idiopathic), like it producing irritating and debilitating effects. This view, which is Andral's, is adopted by Mr. Erichsen and Dr. Markham.

" 9. It may appear a contradiction to the above statements, that, in the more sthenic idiopathic fevers, and in certain pyrexia attending on inflammation, especially in the case of serous membranes, the radial artery is not paralysed, but rather contracted; the pulse is hard or wiry. This would not certainly prove that the swollen arteries are in the same state, but let it be granted that it does. Observation of the results of dividing the sympathetic nerve in the neck shows that relaxation of arteries and increase of temperature are not inseparably connected. The hyperæmia which ensues after the operation diminishes considerably in a day or two, though the elevation of temperature persists. Moreover, in repeating the experiment upon a cat, I found that the temperature became greatly elevated, without the existence of much apparent hyperæmia. It certainly did not appear that the increased heat could be accounted for by the hyperæmia. It is Bernard's opinion that the temperature is not augmented solely in consequence of the part lying in the range of the paralysed nerves receiving more blood, but that there is actually an altered state of the nutrition of the part. Brown-Séquard and Walter, on the other hand, ascribe the increased temperature solely to the increased afflux of blood. The circumstance that the temperature of the side operated on is sometimes 2° or 3° Fahr. higher than that of the internal parts is materially in favour of Bernard's view. So are also the phenomena of phlegmasia dolens, and some analogous *white inflammations* (as Dr. Graves calls them), in which at the same time that there is swelling and increased heat, the pallor certainly indicates that the arteries are constricted. In some cases it may be that the vaso-motor nerves are so affected, so debilitated, that

* For remarks on this important distinction, see Trousseau, 'Materia Medica,' vol. i, p. 42.

increased heat is produced by the derangement and hurry of the nutrition-processes, while yet the arterial muscles retain power enough to be stimulated to contraction by the overheated blood. Their contraction, then, might be explained in the same way as the increased action of the heart.

“10. The accelerated action of the heart in fever (*i. e.*, the increased rapidity, not force, of its contractions) may very plausibly be explained by considering the medulla oblongata or pneumogastric nerve to be in a debilitated state. It is not indeed so fully established that section of the pneumogastric causes increased frequency of the heart's action, as it is that stimulation of it or of its origin retards or arrests it; but the conclusion is at least eminently probable. The rapid action of the heart, the palpitation, and breathlessness, which occur in anæmic girls, and males not anæmic but of weak tone, on any sudden muscular exertion, are more probably owing to debility of the par vagum and medulla oblongata than to any other cause. If a man in vigorous health attempt a feat of running or swimming, without having practised for a length of time, he will surely find that his ‘wind’ speedily fails him; he will have much palpitation and panting; but if he is in training, he can bear the exercise without being blown. In this case, the improved power can scarcely be attributed to anything except the increased energy of the nervous system. The hurried action of the heart, as is well known, is in no wise of the essence of fever; paresis of other centres than the regulating cardiac is necessary to produce this effect.

“11. In the more sthenic forms of fever and inflammatory pyrexia, the diminution of the cutaneous and urinary secretions, and of the salivary, is a phenomenon sufficiently constant to require notice. It stands in sharply defined contrast to the profuse flow which is common in states of debility. In various conditions, of which low nervous power is a prominent feature, a copious flow of aqueous urine is a common occurrence; and its connexion with some depressing mental emotion is often very apparent, as in the hysterical paroxysm, or the case of hypochondriasis related by Sydenham (Sydenham's Society's edition, vol. ii, p. 93). Profuse sweating during sleep is a common occurrence in aguish disorder, without any organic disease or regular fit. Salivation may occur as one of the manifestations of malarious disease. In the case of the kidneys, it is certain that there is not solely increased activity of the glandular tissue, but that the homogeneous membrane of the Malpighian tufts must be in some way altered, so as to allow the more free permeation of aqueous fluid. The same is probably the case with other glands. Now, in the sthenic febrile state, the reverse prevails; the homogeneous membranes are much less permeable by water than usual. In the sthenic fevers, again, this retention of aqueous fluid is not observed; indeed, the limiting membranes allow the transudation not only of water, but also of albuminous matter and fibrine dissolved in it, and even of blood. On what this difference in the filtering power of the limitary membranes depends, is quite a matter of guess; it seems, however, not improbable that it is in part dependent on the amount of fibrine in the blood. In the sthenic inflammations, the amount of fibrine is notably increased; and in these also the diminution of the secretions is, as a rule, most observable. The fibrinous casts of the renal tubes are often so purely

homogeneous, that the idea seems naturally suggested that the limitary membranes may be strengthened and thickened by additions of this substance when it is circulating in excess. It is, however, certain that the filtering power of these membranes may be notably affected by variations in nervous influence. There appears to be a general accordance between the behaviour of the arterial coats and the capillary walls. In relaxed states of the contractile coat of the arteries, the capillary membranes are more permeable than usual, and *vice versâ*. The relaxation of the latter may proceed to that extreme degree in which they allow blood-corpuscles to pass through the softened texture, and ecchymosis or hæmorrhage occurs. I have seen this twice in aguish disorder, as sub-conjunctival effusion.

"12. The liability of the various organs and tissues to asthenic inflammation during the course of fever probably depends on their vital power having been so lowered by the action of the poison that a little hyperæmic afflux becomes a cause of irritation. The case is the same as when a part has been frostbitten, and the circulation has been restored too rapidly. On the same ground, when the sympathetic is cut in the neck of a debilitated animal, severe conjunctivitis sometimes ensues, because the enfeebled tissue cannot withstand the stimulus of the hyperæmia, intensified by the loss of the influence of the vaso-motor nerves. The diarrhœa of intestinal typhoid, and its follicular deposits, seem to me to be well explained in this way.

"13. An occurrence which I think is more frequent in malarious fever, but which Dr. Corrigan speaks of as not unfrequent in typhus, or rather as a sequela of typhus, of late years, is jaundice; the skin and urine being deeply tinged, though the flow of bile into the intestine goes on freely. In this case it may be presumed that the hepatic plexus is paralysed, and the liver in the same state as the side of the head when the cervical sympathetic is divided. Bile is secreted in excess, and a secretion-flux takes place. This, however, would not occur if the vitality of the liver were much depressed; the result would be rather inflammation. Turpentine, which cures the jaundice, acts no doubt in the same way as when it arrests a gastric hæmorrhage, stimulating the relaxed vessels to contract through the medium of their nerves.

"14. The treatment of fever is to be ruled essentially by discriminating observation of the predominant pathological state, whether this approach nearer to excitement and irritation, or to pure debility and asthenia. In the former case it may be needful to bleed largely, as in tropical fevers (*vide* Dr. Copland, 'Fever', p. 930); to give tartar-emetic and opium, as Graves did in typhus with violent delirium; or apply leeches to the temples, as Dr. Corrigan recommends in states of insomnia; or, as we so often do in the diarrhœa of typhoid, to leech the region of the cæcum, and give Hydrargyrum cum cretâ and Dover's powder. All these are instances where lowering means are employed, with guarded caution, to quiet excited action. In the latter case, quinine, wine, and brandy, are to be administered freely, according to the requirement of the individual case. Dr. Stokes', Dr. Brinton's, and Dr. Shute's recorded experience is quite in accordance with my own. I may add, that I cannot but think the old idea that brandy and wine act beneficially by their stimulant effect on the nervous system, is far more

satisfactory than the chemical notion that they act by affording a ready fuel to the respiratory process. It is the nervous system which most of all feels the effect of the poison; and it is reasonable to address our remedies chiefly to it.

“15. In the treatment of pyrexia attending on local inflammation, it is impossible to prevent the effect while the cause continues. Any tonic or stimulant that could be administered to stay the paralysis of the vaso-motor nerves, would act injuriously as an irritant upon the inflamed tissue, impede excretion, and increase the existing mischief. Thus, in febrile eczema, arsenic aggravates the skin-disease and the associated pyrexia. As long as the inflammation is of sthenic character, we must apply our efforts to reduce it; in the case of eczema, we leech the skin, or apply lead lotion, and give saline aperients. When, however, the inflammation has become asthenic, or is so from the first, there is either no fever, or it will yield with the inflammation to tonics. Thus, non-febrile eczema is often cured by arsenic, which probably tones the cutaneous vaso-motor nerves, and so contracts the arteries of the inflamed part. The distinction between the sthenic and asthenic inflammation, as to the effects of treatment, is all important, and seems sadly lost sight of in the bloodletting controversy.”

ART. 18.—*On the nomenclature and classification of Continued Fever.*
By Dr. MURCHISON, Assistant-Physician to the London Fever Hospital.

(*Edin. Med. Journal*, Oct. 1858.)

One reason why many still refuse to admit the plurality of species of continued fever, is their neglect of the circumstances under which fevers originate. In a recent essay Dr. Murchison endeavours to prove that the class of continued fevers comprises three, or in all probability four, distinct species, originating from widely different causes.

First, there is *typhus*, the grand predisposing cause to which is destitution; while the exciting cause, or specific poison, is generated by overcrowding of human beings, with deficient ventilation, and afterwards is propagated by contagion. Hence it is that epidemics of typhus occur during seasons of famine and in besieged cities; and hence it is that we find it limited to the most overcrowded localities of large towns, and seldom meet with it in country districts, or in the upper classes of society.

Secondly, there is the “*relapsing fever*,” about which there may still be some doubts as to its specific distinction from typhus. There can be no question that it differs widely from that disease, both in its symptoms and mortality; and also that a previous attack of the one confers no immunity from a subsequent one of the other. On the other hand, relapsing fever is found to prevail, as epidemics, at the same times and under the same circumstances, as typhus. Researches are still wanting as to the distinctive etiology of these two fevers; but he has grounds for believing that it will yet be shown that relapsing fever is produced by famine alone; typhus, by destitution and overcrowding combined; in other words, that destitution and starvation are the predisposing causes of typhus, the exciting causes of relapsing fever.

Thirdly, there is *typhoid* or *enteric fever*, which is less contagious

than either typhus or relapsing fever, and which is quite independent of the causes which give rise to these, being generated by the putrid emanations from decaying organic (animal) matter. The grounds for this opinion may be briefly summed up as follows :

1. Previous attacks of either typhus or relapsing fever confer no immunity from subsequent attacks of typhoid fever (and *vice versâ*).

2. There is no authenticated proof that the poison of typhus has ever generated typhoid fever (nor *vice versâ*).

3. Typhoid fever does not prevail in wide-spread epidemics. It is essentially an endemic disease ; or when it does become epidemic, such epidemics are always of the most limited and circumscribed character.

4. Typhoid fever is always most prevalent in autumn, or after a long continuance of hot weather. A hot autumn after a wet summer appears to afford the most favorable conditions for its development.

5. Typhoid fever is not, like typhus and relapsing, limited to the poor, but is met with among poor and rich alike.

6. Typhoid fever is not confined to overcrowded localities, but appears alike in the most dense and in the least populous districts of large towns, and even in isolated houses in the country.

7. There is evidence, of the most conclusive nature, that typhoid fever may result from the emanations from (animal) organic matter in a peculiar state of decomposition. In every instance where "fever" has been described as originating from such a cause, the fever has been typhoid. The reason why this cause is not more generally recognised, is the want of attention to the distinctions between the different fevers. Those who deny that "fever" can be the result of putrid emanations, adduce thousands of cases of typhus and relapsing fever as negative evidence, in the same way as there are not wanting a few who bring forward typhoid cases to prove that fever cannot be the result of destitution and overcrowding.

Fourthly, there is *simple fever*, or *febricula*, which is non-contagious, and arises from such non-specific causes as exposure to the sun's rays, fatigue, surfeit, &c. In its simplest form this fever may terminate in twenty-four or thirty-hours, as in the ephemeral or diary fever of systematic writers ; or it may be prolonged to eight or ten days, as in the ardent or sun fever of tropical climates.

Another reason why the various fevers are not more generally recognised as distinct diseases, is attributable to a faulty nomenclature. The term typhus, though derived from a single symptom, and so far objectionable, is one which is sanctified by great antiquity and a Hippocratic origin. The designation, relapsing fever, is no doubt inappropriate ; for, in the first place, in a large proportion of the cases there is no relapse ; and, secondly, it is hardly accurate to call that a relapse which is a constituent part of the ordinary course of the disease ; yet the name is distinctive enough, and, in the present state of our knowledge, it might be imprudent to alter it. The same remark, however, does not apply to the appellation typhoid, to which I beg more particularly to draw attention. It is one which is not only faulty, but tends to create confusion. It is faulty ; for, first, it literally means "like typhus," and, consequently, it is at variance with all precedent in the scientific nomenclature of natural objects, whenever it is desired to confer designations on distinct genera

or species; secondly, because the same word is constantly employed, in an adjective sense, to indicate a group of symptoms, which may come on in the course of any disease; and, thirdly, a large proportion of the cases to which it is applied exhibit no symptoms of a typhoid character, or resembling typhus. For the same reasons, it greatly tends to create confusion; and indeed I have good reasons for believing that this name has done as much as anything else to make the public and the great body of the profession consider that the affection is merely a variety of typhus. At the same time, none of the numerous synonyms is, in my opinion, more appropriate. For example, it would not be desirable to have a name derived from the abdominal lesion, tending, as such would do, to revive in the minds of many the exploded doctrines of Broussais. A suitable distinctive name for the disease remains a *desideratum*; and after having devoted much thought and attention to the question, the author ventures, in the essay already referred to, to propose one, derived from what he believes to be the cause of the fever. In that essay he collects what, in his opinion, is conclusive evidence that typhoid fever is produced by the putrid emanations from decaying (animal) organic matter; and he therefore suggests for it the appellation of *pythogenic fever*—*πύθογενής*, from *πύθων* (*πύθομαι*, putresco) and *γεννάω*. In repeating this suggestion in a more prominent manner, he does so with considerable diffidence, and is actuated solely by the crying necessity there exists for adopting a more appropriate designation for the disease in question.

The whole class of continued fevers may be said to occupy an intermediate position between the eruptive fevers (variola, scarlatina, and rubeola), on the one hand, and the malarious fevers (remittent and intermittent) on the other. As to the causes which originate the eminently contagious poisons of the eruptive fevers, we as yet know little or nothing; and if we have almost succeeded in eradicating one of these, our preventive measures have no reference whatever to the causes which generate it. The malarious fevers are non-contagious; and although we know little of the intrinsic nature of malaria, we have long known the causes and circumstances which give rise to these, and the prophylactic measures by which they may be in a great measure averted. Hence it is that agues, which in former times were so prevalent and so fatal in many parts of Britain, are now rarely met with. The continued fevers resemble the eruptive in being contagious, though to a less degree, while they are also assimilated to the malarious fevers, inasmuch as we know the circumstances under which they are developed, and the means by which they may, to a great extent, be prevented. The author is fully aware that the doctrines here enunciated are at variance with the deeply rooted convictions of a large body of the profession, who, while they admit that the various conditions specified in this paper may favour the propagation of the specific poison of fever already existing, yet deny that the poison of a contagious disease can be by any such means generated *de novo*. But if this view be correct, how comes it that the same conditions only give rise to one form of disease? If, for example, overcrowding only acts by favouring the propagation of typhus (that it does thus act he is far from denying), how is it that, in temperate climates, it is always typhus which appears as an epidemic in a besieged city, or in an over-

crowded prisons, and not diseases which are notoriously more contagious, such as variola and scarlatina? In fact, with regard to typhus and pythogenic fever, the matter resolves itself into this: if certain conditions are present, we can, with almost certainty, predict the result.

Pythogenic fever may be said to form the connecting link between the continued and the remittent fevers. It is but sparingly contagious. There is also much that is remittent in the history of the course of the disease—so much so, that many of the designations which have been bestowed upon it have reference to this character. There can be little doubt, he thinks, that the *hemitriticeus* or *febris semitertiana* of old writers, which was considered a composite disease made up of a tertian and quotidian intermittent fever, was pythogenic fever; while it is now generally admitted that the so-called “infantile remittent fever” is of the same nature. Again, there are not a few well-authenticated instances of pythogenic and malarious fevers prevailing together, and apparently generated under similar circumstances. Such is the case, Dr. Mercer Adam tells us, every summer in the foul-smelling city of Amsterdam. A most remarkable instance of this intimate relation between the two fevers will be found in an account of the diseases observed in the commune of Guermange, in the Duchy of Lorraine, and presented by M. Ançelon to the French Academy of Sciences in 1845. There are also good grounds for believing that careful dissections will show that many of the so-called remittent fevers of tropical climates are typhoid or pythogenic. Already Drs. Scriven and Ewart, of the Bengal Medical Service, have proved, by post-mortem examinations, the existence of pythogenic fever in India and in Burmah. Dr. Scriven, who studied the characters of the disease under Dr. Jenner, at University College, has given figures of the intestinal lesion, and has written that he has met with several other cases of the same fever since the date of his papers. It will be important to ascertain what are the precise circumstances under which such cases are observed in India, and whether they are not different from what are known to develop the more prevalent malarious fevers. With regard to these last, the common opinion at present is, that, although the putrefaction of vegetable matter is a frequent concomitant, it is not an essential part of the process of the development of malaria. This opinion is founded on the facts recorded by Chisholm and Fergusson; but both the facts and the opinion require reconsideration. Without entering into this question at present, the author would merely record his opinion, that it is highly probable that vegetable putrefaction is the source of malarious fevers, while the putrefaction of animal matter begets pythogenic fever.

The paper concludes with a summary, giving the synonyms of each form of fever (a task of no small labour), with the leading distinctive characteristics.

ART. 19.—*On the use of powdered Charcoal and Magnesia in Typhoid Fever.* By M. DILLE.

(*Rév. de Thér. Méd.-Chir.*, June, 1858.)

The description of the cases in which these medicaments were employed is not sufficiently exact to enable us to arrive at a correct opinion

respecting their value. We cannot tell, indeed, whether the cases were slight or severe. At the same time, we think it not improbable, from what we know of the *modus operandi* of the medicaments—a *modus* which would be at once disinfectant and evacuant—that the effects may have been, as they are said to have been, not a little beneficial.

ART. 20.—*On the action of Calomel in Typhoid Fever.*

By Dr. C. A. WUNDERLICH.

(*Archiv für Physiol. Heilkunde*, New Series, vol. i, 1857.)

In 550 cases of typhoid fever, treated during seven years, Dr. Wunderlich has used calomel seventy-six times. As a rule, this treatment was commenced before the expiration of the first week of the malady, and the cases were always of a marked and severe character. If the cases were slightly marked, if there was diarrhœa, colic, or flatulent distension of the bowels, or if the patient was anæmic or liable to scorbutic hæmorrhage, calomel was not given. A single dose of twenty-five centigrammes, or of twenty to fifteen centigrammes, according to the age of the patient, once or twice a day, was the plan adopted.

Of the seventy-six patients who took the calomel eleven died—a proportion which, according to the author, is below the average mortality in this affection. Moreover, seven of these eleven deaths were exceptional; thus, four did not have the medicine until the best time for its administration had been past by considerably, two were almost moribund before they had it, and the remaining one died of an accidental illness contracted during convalescence. Excepting these seven cases, then, the proportion of deaths falls to not more than four in sixty-nine, or 5 to 6 per cent.

In no instance could any injurious influence be attributed to the calomel—neither diarrhœa, nor flatulence, nor head-symptoms, nor feverishness. Slight and transient ptyalism was the only inconvenience, and this was by no means present in all cases. Comparing these cases with those in which this disease was left to its own course, Dr. Wunderlich considers that the calomel was decidedly beneficial. In twenty-five cases, he thinks that the intensity of the malady was decidedly diminished, and that in five the course of the disease was actually cut short. He thinks, also, that in the other cases the patient was not only not harmed, but benefited, and that in all cases the follicular disorder of the intestine was checked by the use of the calomel.

ART. 21.—*Some observations on the treatment of Smallpox.*

By Mr. MANDEVILLE, Surgeon to 3d India Depôt Battalion, Chatham.

(*Dublin Quarterly Journal of Med.*, Aug., 1858.)

Out of thirty cases treated in this way, the author tells us, only one proved fatal. Of these cases, moreover, more than half were confluent, and about ten of an aggravated character, the throat being in all more or less affected, and the greater number having been subsequently

complicated by the formation of numerous abscesses over the body, the face excepted.

"As is the custom in all military hospitals, fever cases are immediately admitted when they present themselves; and during the prevalence of the small-pox the first symptoms of any eruption were carefully looked for, and the more especially in the present instance, as both rubeola and scarlatina were also then prevailing; and upon ascertaining that the eruption was smallpox, the patient was removed into the hospital set apart for that disease.

"The treatment up to this time was the same as for common continued fever, but I now commenced what may be termed the specific treatment. The patient was directed to take a mixture composed of two drachms of compound rhubarb powder, two drachms of tincture of hyoscyamus, and seven and a half ounces of camphor mixture, in doses of two tablespoonfuls three or four times a day; but if the attendant fever was very high, there were added two ounces of the solution of acetate of ammonia to the above. This mixture was generally continued as long as the fever lasted, which was usually about the time when the pustules had fully matured and the scabs had formed, the fever seldom extending beyond that period. It was then omitted, and bark ordered, if there had been great prostration, after a serious attack. A draught, containing one drachm of tincture of hyoscyamus in an ounce of camphor mixture, was given at night, as soon as the patient complained of any restlessness or itching, which was about the second or third day of the eruption in the confluent form; but they sometimes complained of it on the first day, when the eruption appeared to be retarded, as if struggling to force its way through the cutis vera. This was continued every night as long as the sleeplessness from the pruritus continued, and at the same time the following liniment or ointment was laid on with a large-sized camel-hair brush three or four times a day over the face and any other part of the body of which the patient complained of being itchy, viz., half a drachm of extract of belladonna, rubbed up with half a drachm of spermaceti ointment, to which were added three and a half ounces of olive oil and two and a half drachms of chloroform. The cerate was added to give it consistence and hold the chloroform in suspension, but the bottle must, notwithstanding, be well shaken before using, as the chloroform will subside after standing for any time.

"In giving the above as being generally sufficient, I should have said, as the basis of my treatment, I wish it to be distinctly understood that I do not mean it in an empirical sense, as every case of smallpox must be treated according to the individual symptoms, and the practitioner must be prepared to meet every contingency or complication of other organs that may arise, either of the head, chest, or abdomen, and act accordingly; but, as far as I can at present judge, it appears to me that by adopting the above mode of treatment, the tendency to such complication will be much diminished or modified.

"I may here say a few words on the rationale of the treatment. The mixture is intended both for the purpose of keeping the bowels free, and reducing the fever and irritation of the skin without depressing the system, as we cannot tell *à priori* what secondary affection may set in,

in which the vital powers may be brought to the lowest ebb and require to be sustained by enormous stimulants.

“As to the administration of the full doses of hyoscyamus at night, I wish especially to direct attention to it as a curative means in the treatment of smallpox, and I consider that without its aid I should not have had anything like the success which I have had; and there need be no apprehension of giving it even when delirium sets in (the latter being met by appropriate treatment)—the object which I had in view being both for the purpose of procuring sleep, as also of allaying the irritation, and which is gained, I suspect, by its causing the eruption to force its way more kindly through the cutis vera; and this latter effect is also favoured by the external application of the chloroform and belladonna ointment, which I consider a valuable adjunct in the treatment of smallpox. The patients invariably expressed their sense of relief soon after its application, some saying it made the skin pliable; others, that it made it feel cool; others, again, that it made it moist; but all felt it relieve the itching or tingling feel.

“One remarkable fact which I have observed since I have commenced using this ointment is, that none of the confluent cases were pitted (with the exception of three cases, and in these very slightly indeed), and we could perceive, after the scabs became detached, the places where the original pustules were had an elevated instead of a depressed base, so that the subsequent absorption left the skin in its normal condition. I should, however, wish that other practitioners would try the ointment, in order to ascertain whether this latter circumstance was accidental or not.”

ART. 22.—*Incubation of Measles.* By Dr. KERSCHENMEISTER, Assistant-Physician in the Hospital for Children at Munich.

(*Prager Vierteljahrsch. f. Prakt. Heilk.*, Bd. 1, 1858.)

Dr. Kerschenmeister endeavours to fix the period of incubation in measles by observing the day in which the eruption made its appearance in families when a *second* child was affected, and of counting the number of days between this time and the appearance of the rash in the case of the child first affected. Great care was taken to select these cases, in which the infection could have been derived from no other source, and the result was, that in 37 cases, so selected, the rash came out between the tenth and twelfth day in 34.

ART. 23.—*Relation between Scarlet Fever and Measles.*

By Dr. R. KÜTTNER, of Dresden.

(*Journ. für Kinderkrankh.*, March and April, 1858.)

Dr. Küttner is of opinion that scarlet fever and measles are to be regarded rather as varieties of one and the same disease, than as distinct diseases, and in support of this opinion he relates a number of cases which appear to be of a transitional character—cases often described as rubeola. In one case, observed by himself, he thinks he has grounds for

concluding that the same contagion produced measles in one case and scarlet fever in another.

ART. 24.—*On the treatment of Scarlatina with Tincture of Iron.*

By Mr. H. MEADE, Surgeon to the Bradford Infirmary.

(*Medical Times and Gazette*, June 26, 1858.)

“During the last eight or nine months,” says Mr. Meade, “scarlatina has been very prevalent in this neighbourhood, and in some localities very fatal. My attention having been thus directed to the disease, I have been particularly struck by the close resemblance between many of its symptoms and those of erysipelas,* and I determined to treat it in the same manner.

“I had long been convinced of the value of the tonic and stimulant treatment in all forms of the latter disease, and formally placed my chief confidence in ammonia: I found, however, that the mineral acids with quinine were more efficacious, and generally prescribed them, until a few years back, when the tincture of sesquichloride of iron was recommended. Though the value of this remedy has been doubted, I have found it so useful, that I regard it almost as a specific, both in the idiopathic and traumatic forms in the disease; and invariably prescribe it both in hospital and private practice; and I have been assured by other medical men that they have equal faith in its virtues.

“Having so much confidence, therefore, in the tincture of iron in erysipelas, I determined to try it in scarlatina, and I have, accordingly, given it during the last winter and spring to every case that I have seen, with the exception of a few, which were so slight as scarcely to require any medicine. The success of this treatment has exceeded my expectations, and I have only had one fatal case since I commenced its use. Several cases, in which the symptoms set in with severity, were apparently cut short by it; and almost all the cases in which I gave it recovered with unusual rapidity. I give it in doses varying from five to fifteen minims, according to the age of the patient, every three or four hours; and when the throat is ulcerated I also apply a solution of nitrate of silver to the fauces. Several of my medical friends have tried the tincture of iron at my suggestion, and have reported favorably of its use.

“I know that it requires a much more extended experience before the peculiar efficacy of this remedy can be established; and the principal motive that I have in bringing it before the profession is to induce others to give it a trial.”

ART. 25.—*On the shedding of the Teeth and exfoliation of the alveolar processes after eruptive fevers.* By Dr. JAMES SALTER.

(*Guy's Hospital Reports*, Third Series, vol. iv, 1858.)

During the past few years Dr. Salter has had under his care, prin-

* I am aware that there is no novelty in this fact, some pathologists thinking that a most intimate connexion exists between the *materies morbi* in these and some other acute contagious diseases. (See Dr. Holland's ‘*Medical Notes and Reflections*.’)

cipally among the out-patients at Guy's Hospital, a number of cases in which necrosis and exfoliation of the alveolar processes of the maxillæ, accompanied by shedding of the contained teeth, has been one of the secondary consequences resulting from the attacks of the eruptive fevers—scarlet fever, measles, and smallpox. These cases have been so singularly uniform in their origin, course, and entire history, that though following different forms of eruptive disease, they would seem to have a generic identity: indeed, so similar have they been, that it would be impossible, from a mere inspection of the patient during the period of exfoliation, to have any idea as to which of the eruptive fevers had preceded the then condition—a circumstance which, while it simplifies the description of the several cases, seems to indicate that the immediate cause at least, whatever it may be, is common to the several forms of eruptive fever after which this condition occurs.

But though there is so complete a similarity in the cases themselves, the proportion in which they occur, as sequelæ to the previous fevers, differs very considerably. After scarlet fever he has had some eight or ten cases, after measles three or four, and after smallpox only one. These figures, referring as they do to sources of observation, combining private practice with a large population of poor who seek assistance at Guy's Hospital, and this extending over a period of six years, may probably be taken as at least an approximation to an expression of what is the relative frequency with which such consequences succeed the different eruptive fevers respectively.

These cases are all singularly alike, and thus the narration of one case will convey a good idea of them all. Take for example the following: About two years since a poor woman brought her child to me among the out-patients at the hospital, suffering from this tooth-shedding and necrosis of the lower jaw. The child's history was this: She was a remarkably healthy little girl of five years old; about two months previous she had a mild attack of scarlet fever, which had run its course, and passed off without any untoward symptoms; her recovery was complete, and her health restored to its usual vigour. A few days before the mother applied at the hospital, she had, for the first time, noticed that the child's breath had become offensive, and, upon examining her mouth, had discovered appearances for which she now sought advice. The child now looked in remarkably good health, was rosy and robust, and all she complained of was a slight discharge from the edge of the gum on the left side of the lower jaw, at a part corresponding with the temporary molars, and of some slight pain in that region. Upon inspecting the mouth, it presented certain characteristic appearances: both on the inside and outside of the temporary molars, on the left side of the lower jaw, the gum was stripped for the depth of about the eighth of an inch, leaving bare so much of the alveolus, while, from within the edge of the mucous membrane, which was red and tumid, oozed a discharge of pus; and the odour of the mouth was fetid. It was particularly observed that there was no thickening of the jaw, and no effort at the formation of supplemental bone; neither was this necrosis of bone associated with fistulæ or sinuses; the discharge of pus coming simply from the edge of the gum, where it had peeled from the alveolus. The mother of the child did not associate this condition with

the previous attack of scarlatina, and was somewhat surprised when asked as to *which* of the eruptive fevers (naming them) she had recently suffered from.

It was directed that nothing should be done in this case beyond the occasional washing of the mouth with a dilute solution of trishypochlorite of lime, and that the patient should see me again in a week.

On presenting herself at the appointed day, the peeling of the gum from the alveolar process was found to have progressed considerably, not laterally, so as to involve bone corresponding to other teeth, but simply stripping more from the bone already partially laid bare. It was now found, too, that the same series of changes had commenced on the other—the right side of the lower jaw, and to exactly the same extent of lateral boundary, though as yet it had made but slight progress. This state of things gradually and uniformly progressed; the left side being in advance of the right—the discharge increasing, and the odour of the mouth becoming more offensive, till about six or eight weeks had elapsed, when the sequestrum on the left side commenced to loosen, and in a few days was readily removed by a pair of dressing forceps, accompanied by the contained teeth. After the sequestrum was removed, it was found that the base of the jaw was still entire, and the integrity of the maxillary arch unbroken; the bone having ulcerated across beneath the loculi, occupied by the forming bicuspid. The now exposed surface exhibited bleeding granulations, which speedily healed. In about a fortnight the same occurrence took place on the opposite side of the jaw, without any variation in the attending circumstances.

This history may be taken as a typical example, and as illustrating the ordinary course and issue of these cases. Uniform, however, as they are in all essential particulars, they are, nevertheless, subject to considerable variations in degree of severity. Thus, in one case, not only were the corresponding sides of the same jaw attacked, but after the sequestrum and contained teeth had been cast off from the lower jaw on either side, the same occurred in the upper, so that all the eight temporary molars, and all the eight immature bicuspid, with their containing alveoli and loculi, were exfoliated.

The first evidence of exfoliation has always been apparent within eight or nine weeks after recovery from the eruptive fever, usually within four or five. It has never been preceded by swelling or pain, or accompanied by periosteal abscess; the suppuration always occurring at the part where the gum peels from the alveolus, which appears to be the simple method of exposing the dead bone.

It has happened more frequently in the lower jaw than the upper, and when in both, first in the lower.

Whichever jaw has been the subject of the exfoliation, it usually occurs on both sides, either coincidentally, or in rapid succession; its tendency is to be symmetrical.

The severity of the previous attack of fever seems to have no relation to the subsequent exfoliation—a very light attack of eruptive fever may be followed by a considerable exfoliation, or the reverse; sometimes it is associated with other secondary symptoms, more often not; indeed, it has seemed to me to have generally occurred in very healthy children.

The age at which these exfoliations have occurred is worthy of

especial note. It occurs during the time that the most active tooth-development is going on in the jaws, and when all those parts are undergoing the most rapid nutritional changes—about five or six years of age. From four to eight years are the extremest limits noticed.

It is not a little remarkable, that in *every* instance that has come under my observation, the temporary molar or molars, and the corresponding bicuspid or bicuspids, with their containing alveoli or loculi, have been the parts to suffer.

In no one case has the shedding of teeth been confined to the temporary set; the successional (bicuspids) have always been involved, and shed also.

What *may be* a coincidence—Dr. Salter has frequently found these cases among the poor; and what, no doubt, *is* a coincidence, the patients have generally been girls.

The explanation of these cases Dr. Salter considers will be found in a rational interpretation of the three following propositions:

1. Certain diseased conditions of teeth are sufficient to produce their own shedding, by the necrosis and exfoliation of their containing alveoli.

2. In the eruptive fevers the poison of the disease spends its chief force upon the tegumentary system.

3. The teeth are modified papillæ—are members of the tegumentary system.

These propositions are acknowledged truths. By applying them to the cases under consideration, I submit that they explain them—that they solve the difficulty of establishing one similar *immediate* cause as arising from three several *primary* ones, and show why these cases of tooth-shedding and alveolar necrosis, produced by three forms of eruptive fever, may be identical in aspect, identical in course, identical in issue.

ART. 26.—*On the pathology of Rheumatism.* By Dr. F. T. BOND.

(*Midland Quarterly Journal*, April and July, 1858, and *Medico-Chir. Review*, Oct. 1, 1858.)

Dr. Bond analyses the prevailing doctrines regarding the intimate nature of rheumatism, and objects, with regard to the lactic-acid theory, which may be said to be the one most generally prevailing at present—1, that lactic acid has not been shown to be in excess in the blood of rheumatic patients; 2, that, even supposing it to be present in excess, it would be difficult to trace the connexion between this circumstance and the exudations in and about the different fibrous structures of the body; 3, that other acids being in excess in the secretions, and therefore possibly in the blood, they may be as much the cause of the phenomena as lactic acid; 4, that, in regard to the theory attributing the disease to suppression of the cutaneous excretions, it is doubtful whether it is preceded by greater suppression than the prodromata of all inflammatory diseases bring with them; and 5, that the extreme tendency to sweating which occurs during an acute attack of the disease, may be much better explained by another theory.

In order to establish a theory of rheumatism, Dr. Bond next analyses the phenomena of the disease, and finds that fatigue, exposure to cold,

mental emotions or some other depressing agent, exercise a paramount influence in its production; febrile symptoms making their first appearance, followed by local affections in some fibrous tissue. A hyperinotic condition of the blood exists from the first, and the excessive fibrin having a special affinity for the fibrous structure, is specially deposited in and about them; hence the joints and the valves of the heart become the chief seats of the local affection. The preference shown in different cases for particular joints depends upon their greater weakness, or upon their labouring under some abnormal condition, upon the principle enunciated by Mr. Paget, that the depressed nutrition of a joint makes it more liable than any other part to be the seat of inflammation excited by the diseased blood. Dr. Bond's theory, then, reverses the order in which the different constituents of the diseases are commonly supposed to stand. Instead of regarding the hyperinosis merely as an effect of the reaction of the local disease upon the system at large, he considers it to be the primary source of the exudation, the causative agent of the latter, without which it could never exist. The increase in the urinary and cutaneous secretions, and the greater amount of urea, uric acid, lactic, phosphoric, and other acids in them, the author attributes to the metamorphosis of the fibrin; these substances being the products of the degradation of fibrinous matter, "the relations of urea and uric acid to highly nitrogenized matters—as exhibited by the experiments of Lehmann, by the recent manufacture of urea by oxydizing albuminous substances by M. Béchamp, and by the general excess of these excreta in the hyperinotic states of the blood, combined with that of lactic acid, to the muscular juice as determined by the researches of Liebig—amply corroborate this statement as far as these three bodies are concerned; the others, from the smallness of their amount, may be put out of consideration."

Dr. Bond considers the sources of an excess of fibrin in the system to fall under three heads—1, as a result of imperfect primary assimilation; 2, as a result of a metamorphic process, normal in nature, but extreme in amount; 3, as a result of defective elimination of the fibrin by the excretory processes provided for the purpose.

Having said thus much, we must refer our readers for the conclusions which the author draws as to treatment to the paper itself; we will merely add, that his theory possesses a great resemblance to that propounded by Mr. Toynbee, a short time back, at the Medical and Chirurgical Society, shortly after the publication of the first part of Dr. Bond's paper.

ART. 27.—*On Diphtheria.* By the Registrar-General.

(*Quarterly Return of the Registrar-General, March, 1858.*)

"A disease, which is not new, but has been described afresh in France, has been fatal in several districts. It has been called 'throat-disease' in some of the returns, and from its having attacked English visitors in Boulogne the name of that town has been occasionally employed to qualify the affection. Diphtheria, its name in the statistical nosology, is adopted from the French writer who described the disease under the name of *diphtherite*, in reference to the characteristic membranous exudation in

the throat.* The termination "*itis*," as in gastritis, is used in medical language to designate pure inflammation of the organ, which the root of the word expresses; hence *ia* has been substituted for *ite*, the French form of "*itis*," as this cannot with any propriety be placed after *diphtheria*, designating a product of disease, and not an organ of the body.

"No notice has been taken of the disease by the registrars either in the country north of Staffordshire or in Wales; and it has probably not prevailed there epidemically to any great extent. It is, however, allied to one of the forms of scarlatina, and is still confounded with that disease, with croup, or with quinsy, by some practitioners.

"Diphtheria, like Asiatic cholera, is probably only a more intense form of an old disease; but new intense spreading forms of disease deserve close attention, for with the increasing density of population, the intimate connexions between England and every unhealthy climate of the world, and the slow progress of sanitary improvement, we cannot consider ourselves absolutely safe from an eruption of some epidemics, which, like their predecessors, may open a new chapter not only of medical but of national history; for Niebuhr acutely remarks, that the great epochs of history are marked out by pestilences.

"Epidemics, like new varieties of animals, spring up under favorable circumstances. Each epidemic form has its congenial climate. The cholera epidemic is bred on the delta of the Ganges; yellow fever on the banks of the Mississippi; plague around the Nile in Lower Egypt; typhus in our towns; ague in our marshes; diphtheria, according to the popular theory in France—where the conditions are more favorable, on the whole, than they are in England—to the diffusion of putrid effluvia over the fauces.

"Every Englishman admires the works of art, the picture galleries, the houses, the furniture, the cultivated personal tastes which surround him on every side in Paris, or on a small scale in Boulogne. He admires some of these objects every day, others every week; but has every day to give up his admiration at the door of that inscrutable *cabinet* where the light of French refinement never comes; where his throat is assailed by the poisonous distillations that engender disease, and explode, if you count well the victims, with much more fatal consequences than gunpowder or even than fulminating quicksilver. That men should lock up jewels in cabinets, keep their larders full of delicacies, or stock their cellars with wine, is natural; but it is a singular absurdity in civilised men to attempt to hoard for years this volatile essence, which bursts its chains, and, like an unclean spirit, enters not only every apartment in the house, but every channel of access to the living chambers of the body, leaving at times such traces of its passage as diphtheria in the throat. The disease once generated, wanders abroad, and destroys life under circumstances quite different from those in which it was born; but impurity is always its natural ally.

"The Scotch threw these matters into the streets, and justly incurred the censure of the fastidious. In London, and even in the country mansions of England, retreats still exist which may rival the French

* Diphthera—διφθέρα—a prepared hide, leather. διφθεραι were used for writing on in the east, like vellum or parchment. (Liddell and Scott.)

magazines of impurity; but it has of recent years been the practice to throw the guano compounds of London, with water, into the sewers; which, though not constructed for the reception of such matters, and consequently suffering their volatile principles to escape into the streets, convey a portion of their elements to the Thames, and commit them to its flood of tidal waters.

“Dr. Barker has recently performed an ingenious series of experiments on animals, to determine the effects of each of the noxious principles which arise from cesspools. He placed the animals in a close chamber by a cesspool, with which a tube opening into the chamber communicated; and a lamp was arranged so as to draw a current of cesspool air steadily over the creatures inside. With a pair of bellows Dr. Barker could draw the air from the chamber. A young dog in half an hour ‘became very uneasy and restless; he vomited, and had a distinct rigor, and in the course of a day was exhausted. When he was removed he soon recovered.’ ‘Another dog was subjected to the cesspool air during twelve days:’ in the first seven days he underwent a series of sufferings, not unlike the symptoms of the disease of children in hot weather; on the ninth he was ‘very ill and miserable.’ After he was liberated, on the twelfth day, he remained ‘very thin and weak for six weeks.’ Dr. Barker then continued his experiments on the effects of definite doses of the gases in the sewers, and killed or poisoned several sparrows, linnets, jackdaws, and dogs.

“Thus Dr. Barker has, for our instruction, imitated on a small scale, and on a few of the inferior animals, the vast experiment which is constantly going on, and destroys thousands of men, women, and children all over England. Instead of a few animals in a close chamber, more than two millions of people live in London over sewers and cesspools. The poison is generated in every house; it is distributed conveniently along all the lines of road, so as to throw up its vapours into the mouths, throats, and lungs of the people through innumerable gully-holes, which are either left untrapped, or trapped imperfectly, in order that the poisonous gases might escape. A variation in the pressure of the atmosphere draws up the stinking air from the sewers, like Dr. Barker’s bellows. All the details of the experiment were as carefully contrived by the engineers of the old Sewers Commissioners as if they were constructing an apparatus for passing currents of poisonous airs steadily over the people of London, with a view, like Dr. Barker, to ascertain their exact effects. The engineers of the new Board of Works have endeavoured to keep the apparatus in order.

“It is now time that this cruel experiment should cease. Last year, when no epidemic prevailed, not less than 14,795 unnatural deaths were registered in London. This was the aggregate effect of the impure airs, and of other sanitary defects.

“Will the London Board of Works stop the experiment? Are they, like Dr. Barker, convinced and satisfied? Will they bring their common sense to bear on this question? Gases are constantly generated in the sewers and cesspools, and these gases will escape. Their elasticity carries them—and perhaps still more poisonous organic compounds—through the gully-holes, so long as there is no other outlet. But what can be an easier engineering problem than to discharge into the atmos-

phere the sewer gases, through pipes running up, and at least as high as the chimneys? This is in partial operation, and if made universal would be a mitigation of the evil. There are many ways of getting entirely rid of these gases, and why should not the inexpensive work be at once done?

"The sweet odours that enter the country are taxed; and everyone has witnessed the admirable zeal of her Majesty's customs' officers in their searches for eau de Cologne. If a tax could be levied upon odours of another description, bearing some proportion to the evil they do, it would be much more productive; and if it were levied through the agency of the Board of Works in London, and the Sewers Commissions elsewhere, it might be more beneficial, as they would undoubtedly find it economical to substitute fountains of rose-water for their present gully-holes."

ART. 28.—*On Diphtheria as caused by the Oidium Albicans.* By Dr. LAYCOCK, Professor of the Practice of Medicine at the University of Edinburgh.

(*Medical Times and Gazette*, May 29, 1858.)

In the following remarks, which occur in a clinical lecture upon a case of cancer of the supra-renal capsules and mesenteric glands, without bronzing, Dr. Laycock points out an analogy between diphtheria and muguet, and endeavours to show that both these diseases are due to the presence of a parasitic fungus on the surfaces of the mouth, fauces, and other mucous structures.

"The case before us has various points of practical interest. The immediate cause of death was the exhausting intractable diarrhœa. Now this supervened coincidently with an attack of diphtheria or diphtherite. At the onset of the disease, and just before death, we found in the pellicle formed on the tongue and fauces the sporules and mycelium of the *oidium albicans*, a parasitic fungus found also in muguet—the epidemic aphtha or diphtheria of infants in France. This is an interesting fact at the present moment, when diphtherite is prevalent, more especially as the pellicle was also found abundantly after death in the œsophagus. I have little doubt that this pellicle was due to the action of the parasite on the enfeebled mucous surfaces of the mouth, fauces, &c. It acts, like all its tribe, as an irritant, inducing increased formation of epithelial scales and effusion of mucous exudation-corpuscles, or plasma; intermingled amongst these are the sporules and the mycelium of the microscopic fungus; the whole constitutes a pellicle or membrane, as it has been termed, varying in thickness and tenacity according to the surface attacked, and according to the condition of the patient. The parasite seems to act upon the capillaries of the subjacent tissue, as when removed blood is not uncommonly effused and the surface looks raw. Diphtheria is not, however, necessarily limited to one form of disease. We have, in fact, had a case of syphilitic disease of the fauces and pharynx, in which the pellicle containing the *oidium* was noted, and which seems to have introduced it into the clinical wards. Again, if the fungus multiply in a population at the same time that there is an

epidemic of scarlatina or rubeola prevalent therein, that epidemic may be expected to take the diphtheritic form in those cases which are attacked by the oidium. I must add, however, that we have had reasons for thinking that the oidium acting alone will fasten upon the mucous membranes of the mouth and throat, and excite inflammation, and without the formation of a pellicle. Or if it lead to the formation of a pellicle this may be constituted of spores only, with exudation-corpuscles, constituting a tougher membrane than that usually found on the tongue and tonsils, and resembling the pellicle of croup. The diagnosis of diphtheritic oidium from ordinary aphtha is founded, first, on the character of the morbid appearance; for in ordinary aphtha the disease is vesicular, and the white specks or patches are ulcers, while in diphtheria they are pellicular, and not ulcerative, while the redness is much deeper than in aphtha. Besides, the microscope may reveal the spores and mycelium of the fungus. The development of mycelium is, however, by no means a necessary result of the action of the fungus. This seems to be peculiar to the more advanced stages; at first there is not even a pellicle, only characteristic redness of the affected surface. Dr. Young, our resident physician, got an attack of sore-throat shortly after one of the patients affected with oidium coughed in his face, while he was applying a remedy to the patient's fauces. Dr. Young had this characteristic deep-red congestion of the fauces, with but a very limited production of pellicle on the pharynx, in which no mycelium was to be discovered. Further, it is probable that, besides the stage of development, the condition of the *habitat* may make a considerable difference as to the morbid products. Thus, since warmth greatly promotes the spread of the disease in the form of muguet, the absence of mycelium in diphtheritic croup may be due either to the fact that the weather is cooler when it prevails, or that the mucous membrane of the larynx and trachea, being cooler generally, from the transit of air, is less favorable to the development of the mycelium than that of the mouth, fauces, and œsophagus. Again, the condition of the intestinal mucous membrane seems less favorable to the formation of the mycelium, or of a pellicle upon it. Still inflammation and even ulceration of these surfaces will occur as the result of the irritative action of the parasite, in the same way as ulcerative inflammation supervened in the œsophagus of the patient in question. This remark applies also to the bronchial mucous membrane, in which I am inclined to think the oidium may develop an inflammation of the same low type as that seen elsewhere—an asthenic bronchitis with a purulent secretion.

“In France, a cutaneous form of muguet is observed in children, characterised by a vivid or deep-red erythema and ulceration, more particularly of the inner surface of the thighs, the labia, malleoli, &c. There seems no reason to doubt that the fungus (like others of its kind) may fix upon a suitable portion of the skin. A sucking child with muguet will communicate the disease to the nipple of its nurse. In the case of the syphilitic sore throat attacked by the oidium, there were also superficial ulcerations of the face of a glazy unhealthy character, one at the angle of the right eye, and one at the right angle of the mouth, which were treated locally as diphtheria-ulcerations, by a strong solution of borax, and with speedy benefit. How great a share these microscopic parasitic

organisms have in the causation of disease, remains yet to be ascertained ; but I may mention to you, as a curious fact of the kind, that in a case of asthenic bronchitis, admitted a few weeks ago into the wards, the *volvox globator* was found in apparent abundance in the fur of the patient's tongue, which was brown, rather dry, and scabrous.

"The indications in the treatment of diphtheria are twofold. First, to look to the constitutional condition and treat that ; secondly, to aim at the destruction of the parasitic fungus, more especially with a view to prevent its extension along the food and air-passages. As to the constitutional condition when a pellicle forms, it is, I believe, invariably asthenic, arising either from a chronic morbid state or from the action of a depressing fever poison on the blood and on the parts affected, as in scarlatina. Like all parasitic fungi, it thrives best where the vitality is low. Where there is not an asthenic state the oidium seems rather to excite a deep red inflammation of the tonsils and fauces generally, and if a pellicle form, little if any mycelium is seen in it. As a general rule, tonics and stimulants, and the so-called 'antiseptics,' are indicated.

"Locally, the remedies which we term parasitocides are the best. There is of these a wide range of choice, but perhaps the saturated solution of the biborate of soda is the most efficient and the safest. It is a powerfully destructive agent to the class of fungi ; but alkalies generally are parasiticidal. As the oidium is apt to attack the œsophagus, especially in children, and thence extend downwards to the stomach and intestinal canal, the borax may be administered internally also. For similar purposes the chlorate of potass may be used. We applied the linimentum æruginis (an acetate of copper) in the syphilitic case with good effect. The bichloride of mercury, in the form of a gargle, is also a destructive parasiticide, and is an old remedy for 'malignant' sore-throats : but any metallic salt (as the sulphates of zinc, iron, copper) would probably be efficacious. Alkaline gargles and applications, and especially the chlorides (as common salt), have also been found advantageous. Although the nitrate of silver was applied in the case of Denholme, it is not under ordinary circumstances the best application, although the most popular. It is evident that sporules of the fungus may and do pass from one person to another ; or, in other words, diphtheria, as due to the oidium albicans, is infectious and contagious. It is of importance, therefore, to take the necessary measures for the prevention of its spread. In the case of families of children, the best undoubtedly is isolation of the sick from the healthy."

ART. 29.—*Diphtheria and its connexion with a parasitic vegetable fungus.*

By Dr. WILKS, Assistant-Physician to Guy's Hospital.

(*Medical Times and Gazette*, Oct. 2, 1858.)

Opinions still vary as to the true nature of diphtheria, and as to its connexion with a parasitic fungus (*oidium albicans*). As on several occasions the white film on the throat has been found to consist of this fungus, it has been conjectured whether the malady is not really one having a parasitic origin, and the belief has been rendered more

probable from the fact that several new diseases have of late prevailed throughout the organic kingdom, both animal and vegetable, which are clearly traceable to parasites; for example, the oidium of the vine. Unfortunately, those practitioners residing in districts where diphtheria has been endemic have been silent on this point, and it has only been by occasional observers that the fact has been made out. In the few cases of the disease which Dr. Wilks has seen, a fungus has always been present, and thus my belief was, until lately, growing strong that in this observation would be found the true character of the malady, or, at least, that the parasitic growth was intimately connected with it, the question still remaining open whether the formation or growth of the fungus is the primary process, or whether a diseased condition of the surface must not previously exist to prepare a suitable nidus for its development; a question still debated in connexion with other parasites, as the *porrigo lupinosa* (faveuse), a disease in which some cutaneous inflammation is generally found, and thus creating a doubt as to whether this is excited by the fungus, or whether an herpetic or pustular eruption does not previously exist to form a suitable soil for the sporules which are afterwards sown in it. Let the formation in the throat be primary or secondary, it still remains important to know whether its presence is an essential part of the disease; indeed, the spreading character of the pellicle, its separation and destruction by corrosives, are all facts which seem to indicate that many features of the disease are due to its existence.

His attention being directed to this matter, the author took the opportunity to examine the films which occasionally form on the mouths of those sick with various diseases; and on submitting them to the test of the microscope felt some surprise in witnessing in all fungous growth which he has not been able to distinguish from that of diphtheria. Thus, he lately had a woman die under his care in Guy's Hospital, with acute cerebral and spinal meningitis, pleuritis, &c., of a supposed phlebotic origin, and on examination of the pharynx after death, a pellicle was found composed of the parasite. Again, a child four years old presented itself among his out-patients, apparently dying with croup, but on examination was found to be suffering from an extension of diphtheritic disease into the trachea. The throat and tongue were covered with a white pellicle, a portion of which, being placed under the microscope, displayed very readily the oidium; the only difficulty about the case being the statement of the mother, that the child had suffered with a throat affection for several weeks. Mr. Hardy made a post-mortem examination. The throat, trachea, &c., were covered with a pellicle, as before said; and on removing this to find a cause for the chronic symptoms, a polypus of a papillary character was seen growing from one of the vocal cords, with thickened tissue around. Here was an explanation of the chronic symptoms; and upon this had arisen an acute inflammation, accompanied by the fungus. Another case was that of a man who died last week under the author's care, in the hospital, with softening of the spinal cord. A few days before his death his mouth and tongue became covered with a white secretion, which very rapidly formed a complete layer over the whole buccal surface. An examination of this by the microscope showed a remarkably fine spe-

cimen of the fungus, the mycelium and sporules exhibiting themselves to perfection. On mentioning these circumstances to Dr. Barlow, he stated that he had under his care a child with a white film on its mouth (the case not being one of diphtherite), and he sent some of the secretion for examination, when it was found to resemble the specimens already named; and the same occurred in one or two other cases. These facts are sufficient to show that a vegetable fungus may spring up on the buccal mucous surface in various cases of disease, but requiring probably some previously morbid condition for a nidus. Is it not so in diphtherite? Is the disease, strictly speaking, a malignant sore throat, and the formation of a pellicle an accident; or is the latter an essential part of the affection? In the case of the child just mentioned, if no post-mortem examination had been made to discover the chronic disease, the case would have been called diphtheria; and in the man with spinal paraplegia, the condition of the mouth would have been sufficient to have marked it a case of the same kind had there been no other affection present. Such cases may throw some light upon the opinion of those practitioners who, not residing in diphtheritic districts, and who see only isolated cases, regard the disease as a mere modification or peculiar form of some ordinary maladies, as cynanche and scarlatina, and this may in some instances be correct. In speaking of the parasitic growth found in the above-mentioned instances we are aware of the objection which can be made—That the fungus of diphtheria is peculiar (supposing it always to be present), and that found in the mouth of other sick persons is in connexion with aphthæ, and is another variety. In answer, Dr. Wilks says that he failed to discover in the above cases any difference, and, moreover, the character of the pellicle and its rapid extension over the whole mouth, throat, and tongue, was totally unlike ordinary aphthæ.

ART. 30.—*On Erethismus tropicus.*

By Mr. S. R. TAYLOR, Deputy-Inspector of Hospitals, Fort Pitt.

(*Lancet*, Aug. 21 and 28, 1853.)

Under this name Mr. Taylor proposes to collect in our Indian military medical returns all cases of that peculiar disease, or state of system, which exhibits the acute effects of continued high tropical temperature on the European, and which he believes to be an exhausting excitation of the system by continued tropical heat. Sporadic cases of this disease are not uncommon in India every hot season, and occasionally a European regiment is prostrated by an epidemic or endemic outbreak of it.

“The common features of the disease, or state of system, I alluded to are as follows; and in enumerating them, I am bearing in mind instances that came more immediately under my own observation whilst serving in India from 1842 to 1854. The attack is generally more or less sudden, and the symptoms of the premonitory stage are rapidly succeeded by coma, from which the patient rarely recovers. A soldier, apparently in good health, and at some ordinary occupation in his barrack-room; or on sentry, or guard, in a close and very hot, though

shaded place ; or on, or after, a march in the hot season, or hot hour of the day ; or in hospital for dysentery, or some other disease ; or a lady in the cabin of a boat on the Ganges in the hot season, is suddenly seized with overpowering lassitude and feelings of oppression, from the great heat of the weather. There is fulness and distress about the præcordia, perhaps sickness at the stomach ; generally pain in the head or eyes ; the skin is at the same time hot, and ‘a burning heat inside’ may be complained of ; the pulse is more or less strong, according to the general previous condition of the individual. These symptoms last but a very short time ; perhaps in ten minutes from the onset of the disease the patient is insensible, and at the same time he may be convulsed. Deep coma succeeds, and from this stage recovery is very rare. During this coma the breathing may be stertorous and apoplectic, or gasping, or otherwise. The most extraordinary symptom in these cases is the progressive rising heat of skin as the disease advances, and even for some time after death. It must have been an instance of this peculiar affection that Dr. Livingstone met with in Africa. He relates that he was called to see the body of a Portuguese lady, which continued so extraordinarily hot six hours after death that her decease seemed to him for that period doubtful, and he accordingly delayed the funeral till signs of decomposition appeared.”

ART. 31.—*Researches on the Malignant Pustule of man and animals.*
By Dr. R. VIRCHOW.

(*Archiv. für Pathologische Anatomie*, New Series, vol. i, 1857.)

The author has ascertained by experiment that this disease may be propagated by inoculating, not only with the matter of the pustule, but with the blood of the diseased body, and that the time which elapses between the inoculation and death varies between 44 and 66 hours, with one exception, in which a sheep died in 31 hours. He has also investigated with much care the characters of the blood, and found that the white corpuscles are present in greater numbers, with some vibriones. These latter bodies were met with in living blood, and consequently they are not to be regarded as the products of decomposition.

(C) CHRONIC DISEASES.

ART. 32.—*On a method of curing Gout.*
By Professor SERAFINO BELLI.

(*Il Raccogl. Med. de Fano*, and *Charl. Med. Rev.*, July, 1858.)

The following account, which is said to be prepared after many years of observation, is taken from the original by Dr. F. P. Porcher.

Having premised some reflections upon the importance of collecting and publishing our medical observations, Professor Belli gives a resumé of his practical experience, with regard to the treatment of gout, in the following words : “The principal remedy that I employ

in the curative treatment of gout is a purgative drink composed of an ounce or more of sulphate of magnesia, a scruple of nitrate of potash, a grain and a half (*grano*) of sulphate of iron, dissolved in a pound and a half of water. The patient takes, early in the morning, and upon an empty stomach, this drink in three parts, with intervals of a half hour between each; after this he is advised to drink a cup of thin meat broth, or an infusion of malva leaves, of tea, or even of camomile flowers, if he prefers them. The effect produced in a little time consists in repeated evacuations of the bowels, copious, easy, and without pain of any kind; they become very bilious and liquid, finally assuming a colour more and more dark to dark green. I begin the treatment with this purgative, which is continued for several days consecutively, according to the constitution of the patient and his toleration of it. In the more feeble it is ordered thrice a week—interposing a day of repose between each upon which the purgative is used, resuming the same again once or twice a month, at intervals of about fifteen days. After some months, its employment every thirty or forty days suffices. But, I remark, that which is a principal and indispensable part of the treatment is, the advice to my patients to be on their guard to use this purgative drink each time that they have the first indications of an approaching access of gout; for truly I have always and invariably seen, and can assure one upon the faith of my honour, that taking this drink two or three times upon the first appearance of the symptoms of gout, its access will completely and entirely vanish. This fact has been observed by me so often, and with such precise evidence, that I do not hesitate to proclaim it certain. The reason is, I am entirely convinced, that we can any day verify it with constancy and certainty, and with identical results each time that it is subjected to the proof. It is nevertheless very important, and I may say a radical condition of this treatment, that the patient who suffers from the first attack of gout should learn to know and distinguish the symptoms which are the usual precursors. Such are those certain special perturbations in the functions of the stomach, from whence arise want of appetite, tension in the region of the stomach itself or the hypochondrium, flatuosities, feeling of malaise, and of unusual heaviness of body. Some are advertised of it by formication, unaccustomed itching, or even a notable coldness of the lower extremities, in whom also there are even painful cramps, and, as Barthez observes, sensations analagous to that produced by wind blowing along the muscles of the thigh. Baglivi observed, as an almost infallible sign of an approaching attack, the turgescence of the veins of the parts about to suffer. The sleep is interrupted, unquiet, and light, from whence it happens that some are seized with a profound depression of spirits; others are rendered irascible, and give themselves up to attacks of anger upon the slightest cause. Van Swieten said that he knew personally some of the most learned men, and of particularly benign and placid dispositions, who knew with certainty when an attack of gout was approaching, by a change for the worse in their dispositions occurring a few days previous. They have, besides, frequent eructations, borborigmi, and frequent escape of gas per ano. Sometimes there is obstruction, sometimes diarrhœa. In others, the invasion of gout, as was observed by Sidenham, Barthez, and Van Swieten, was only pre-

ceded by a state of unusual good health and increase of appetite, of joyousness, and more marked amatorial propensity. I say, that these symptoms should be well observed and kept in memory by those who are verging upon a first attack of gout, for they will enable them to obtain the earliest indications of its appearance. It would also be a commendable diligence in the physician to apprise patients of this, and urge upon them the above considerations. It is not so necessary for those who have become veterans in this most painful service, for their own sad experience does not permit them to forget.

“The other remedy which I adopt as an auxiliary of the first is the juice of the field chicory (endive succory) taken repeatedly every morning upon an empty stomach, in doses of three ounces, made more mild and operative by an ounce of syrup of the wild strawberry. These are medicines easily borne, and by no means nauseous. But it is necessary to employ it perseveringly each day, and during a year, well understanding that they are to be discontinued when the purgative becomes necessary. They can be used at any stage of the disease. It can be used at every stage, for it is well preserved in the syrup of strawberries. In this section it is impossible to obtain the chicory fresh during the winter, to press out the juice; then it suffices to use an infusion, or even a decoction made with boiling water, on the preserved roots, collected during the spring, and well preserved. This remedy soothes the stomach and materially influences the regularity of digestion, keeping the bowels daily open, and maintaining a salutary influence upon the liver and intestines. Much space would be required to detail all the praise which the most respectable practitioners, and, above all, Galen, attributed to chicory, especially the wild, as being more bitter, vaunting it as an excellent stomachic, diuretic, temperant, or, to use more common expressions, refreshing and laxative. In reference to the strawberry, the observations left us by Linnæus are very valuable—of his having found it a most prompt and efficient remedy in gout, which tormented him bitterly until he ate strawberries in sufficient quantity. [The classical scholar will be reminded of Rousseau’s testimony regarding the beneficial influence of the strawberry season upon his attacks of renal disease. Golding Bird also refers to the vegetable acids contained in this, as very naturally relieving this class of patients.—*Trans.*] This fact I read, some twenty years since, in the ‘*Annali Medico Chirurgici de Roma*,’ published by my distinguished friend, Telemaco Metaxa, whom death, too soon, alas! robbed from the ranks of science, and whom the Roman medical school deplored. And since, when I experimented among those sick with calculus and diseases of the kidneys, upon the utility of the leaves and roots of the wild strawberry, its utility, as observed by many old country people, those most faithful preservers of popular traditions, brought more in mind the observation of Linnæus; and I commenced to use, with much confidence, the plant, both in gout and disease of the kidney, which are known to depend upon the same morbid principle. My confidence in it has never been weakened by experiment, but each day has become firmer.

Here, then, is the whole plan of treatment adopted by me in this disease, and which has thus far fortunately fulfilled all my expectations. It is very simple, unexpensive, easily applied, and accompanied by no

objectionable features. I am in the habit of prohibiting my patients the abuse of certain articles of food and drinks. which are injurious in their effects upon digestion, or in being too heating to the stomach—as, for example, milk, cheese, salt meat, alcoholic drinks, aromatic substances, meat kept too long, &c. But observe, that it is only the abuse of these that I regard as incompatible with the cure of gout. I do not think it advisable to weaken the strength of those liable to it by too severe a diet, or by a certain rigorous abstinence, which first annoys him, and finishes by bringing on a most painful depression, makes him impatient of the restraint imposed, and causes him to throw himself into the opposite course of unbridled excess, regardless of every thought of cure, prevention, or hygiene. For myself, I believe that moderation renders healthful certain articles of food and drink, just as a proper dose of certain poisons is beneficial rather than hurtful. Health lies in temperance. *Abstinence* and *sustenance* were the two precious words, according to Epictetus, which in themselves contained even a moral philosophy. It is in this that the value of the above medicine consists.”

ART. 33.—*Treatment of chronic Rheumatism with Arsenic.*

By Dr. JAMES BEGBIE.

(*Edinburgh Medical Journal*, April, 1858.)

It was at the close of the last century that cod-liver oil was accidentally introduced into practice, in the wards of the Manchester infirmary, as a remedy in chronic rheumatism. It was in the same place, and in a similar manner, some years subsequently, that arsenic was first administered for the cure of that disease. In the writings of Haygarth and the elder Bardsley will be found many cases illustrating its efficacy. In more recent times it has not disappointed expectation. Speaking of the use of arsenic in chronic rheumatism, Dr. Christison says, “I have known several cases of this nodosity of the joints, as some authors term it, get well under the continuous administration of arsenic for some weeks; and it appeared to me that the commencement of the cure concurred with the first development of the physiological effects.” “Arsenic (says Dr. Fuller, one of the latest writers on rheumatism), judiciously administered, and carefully watched in its effects, is one of the most valuable remedies in the chronic forms of rheumatism.”

Many years ago, an industrious workman, approaching the decline of life, applied to me for the relief or cure of the crippling and painful swellings of the small joints of his hands, and particularly of his feet, under which he had long laboured, and by which he had been rendered utterly unfit to pursue his usual avocations. The pains become aggravated at night, and under vicissitudes of temperature, and the patient was sensitively alive to changes of weather. It was with great difficulty and considerable suffering that he had been able to hobble to my door. When I speak of his case as one of chronic rheumatism, I sufficiently describe it. Under remedies external and internal, orthodox and empirical, he had derived no benefit; and seemed almost hopeless of relief. He was ordered to take five drops of the liquor arsenicalis after each meal, and to add one drop every third day till the eyelids became affected.

He faithfully followed the prescription for many weeks, and underwent the trifling disorder which characterises the operation of the drug. He continued his attendance for several months. The knobbiness and stiffness and pain of his joints gradually subsided and disappeared; he walked repeatedly to my house, a distance of a mile and a half, with ease and comfort; he improved in general health; at last he ceased his attendance, he returned to his workshop, and I saw no more of him.

A married lady, in the prime of life, the mother of several children, the descendant of gouty ancestors, and a sufferer in earlier years from painful and disordered menstruation, consulted me last autumn, for symptoms corresponding in some measure with those exhibited by this workman. She was very lame from the stiffness, swelling, and deformities of her toes and ankle-joints, and quite incapable of holding a needle, or directing a pen, from the painful nodosities of her fingers and hands, the distorted appearance of which presented a remarkable uniformity on both sides of the body—symmetrical, in obedience to the law of blood-diseases, as noticed by Dr. Budd. She had feverish, restless nights; a worn-out, emaciated look; a tendency to hectic paroxysms; a depraved appetite; a loaded tongue; along with copious lithates in the urine and considerable derangement of the biliary secretions. These symptoms had supervened on a miscarriage she had suffered in the spring, followed by a protracted and anxious attendance on a near relative during a dangerous illness. The severity of the attack had, in a great measure, subsided before she came under my care. She had been judiciously treated in the north of England, where she usually resided: colchicum, iodine, and various other remedies had been employed; but her disorder went on. After it had been corrected to some extent by depurants and laxatives, without any relief to the local disease, she was ordered to take the liquor arsenicalis in the usual dose, and with the usual instructions. She continued two months under my care; the medicine was taken regularly during that time; no well-marked physiological efforts ensued; but a gradual improvement in the condition of the hands and feet took place; she was able to walk with comparative comfort, and to handle her knife and fork with ease. Her general health had improved, the secretions of the kidney and liver had assumed a normal character; she added to her usual remedy the free use of lemon juice, and an occasional warm bath. She left Edinburgh, with instructions to continue the arsenic, and begin the use of cod-liver oil. I learned, in the course of a month, that she had made progress towards ultimate cure; that the pains, and swellings, and stiffness were gradually subsiding; that a fulness of the eyelids had been observed, along with a dryness of the mouth and tongue; that she had intermitted the use of the arsenic for ten days in consequence of these symptoms concurring with uneasiness and slight pain in the stomach; but that she had failed to witness any of the other phenomena for which she had been directed to watch. I subsequently learned that, with the exception of the short interval referred to, she had persistently taken the arsenic for three months, without any other unpleasant consequences than those alluded to; and had, on the contrary, during the time it was suspended, experienced some increase of the pain, and stiffness of the small joints, which had, however, given way on her resuming

the medicine. She was directed to continue the use of the mineral in diminished doses, and to adhere to that of the cod-liver oil. The last accounts state that she is greatly improved in health, and able to walk with ease to a considerable distance.

The case of the lady differs in some respects from that of the workman. His presented the true characters of chronic rheumatism; hers manifested those of rheumatic gout, as it is called, a painful and obstinate affection to which females suffering from disorder are peculiarly liable. It will be remarked, that in the former case the mineral acted quickly and successfully; in the latter its effects, both physiological and curative, were slowly and imperfectly developed. The cases together, bear witness to the correct observations of Dr. Bardsley, and the earlier exhibitors of arsenic in chronic rheumatism—that while in the one form of disease the medicine will be found to cure without assistance, in the other form it will be necessary to call in the aid of other remedies. Still, in the language of Dr. Fuller, it will be found “a faithful ally.”

ART. 34.—*Coffee and lemon juice in Ague.* By Dr. VON HOLSBECK.

(*Presse Belge, and Montreal Med. Chronicle, March, 1858.*)

Von Holsbeck draws attention to a mode of treatment which he has found useful. Infuse an ounce of well-roasted coffee in three ounces of boiling water, and having strained the fluid, acidulate it with lemon juice. The whole is given at once, five hours before the paroxysm.

ART. 35.—*Calcareous salts in the treatment of Rickets.*

By Dr. WM. BUDD, Physician to the Bristol Infirmary.

(*British Med. Journal, June 7, 1857.*)

Dr. Wm. Budd states, that he has derived better results, in the treatment of rickets, from the compounds of lime, than from any other remedial means. In cases tending to rickets, too, where children with large heads, tumid bellies, and pasty complexion, whose spines are too weak to support their bodies, who are slow in intellect, and backward in teething, and have reached the age of eighteen months or more, without showing any desire to take to their feet, the beneficial influence of calcareous compounds is equally manifest. In such cases, Dr. Budd gives five or ten grains of phosphate of lime, in chalk mixture, thrice daily, adding a simple chalybeate if anæmia is palpably present. At the end of a fortnight, the improvement of the patient is generally conspicuous. Dr. Budd believes that the deterioration in the teeth of the rising generation is due to the insufficient supply of the inorganic constituents of these organs in the food of children, and suggests that children in cities should be fed, in part, during dentition, on biscuit containing a proportion of salts of lime.

ART. 36.—*On the use of ox-gall in Hypertrophies.*

By Dr. BONORDEN.

(Berlin Med. Zeitung, No. 1, and Medical Times and Gazette, Oct. 2, 1858.)

Dr. Bonorden believes that this remedy is much less employed than it deserves to be. It has usually been employed internally only as a resolvent in chronic constipation, and externally in opacities of the cornea. He believes it exerts a special effect on the metamorphoses taking place in the capillaries, and for that reason is highly efficacious in all forms of hypertrophy. In induration and hypertrophy of the *mammary* it exerts a surprisingly rapid effect, and in this way tumours and indurations have been dispersed by him which would have been by others removed by the knife. He usually employs it in combination with olive oil, adding conium if there is pain, and liq. ammon. if there is torpidity. The following formula is very useful: \mathcal{R} Fell. tauri inspiss. \mathfrak{z} iii; ext. conii mac. \mathfrak{z} j; saponis natronat. \mathfrak{z} ij; olei oliv. \mathfrak{z} j; M. and rub in four times daily. Formerly he was in the habit of excising hypertrophied *tonsils*, a practice he has quite left off since he has been aware of the powerful agency of this substance. The gall, rubbed up with water into the consistency of an ointment, is applied by means of a good-sized camel's-hair pencil twice a day to the entire surface of the tonsil. It causes a slight irritation, which lasts about half-an-hour, and is succeeded by an augmented secretion of mucus. Unpleasant to the patient at first, he soon gets accustomed to it; and indurations which have lasted for years give way under its use in a surprisingly short time. In all hypertrophic affections of the *eye*, as hypertrophic opacity of the cornea, pannus, and staphyloma, the ox-gall does good service. Either the fresh gall may be dropped into the eye several times a day, or it may be applied to it with a pencil. In various other hypertrophies, which are accessible to external applications, we may resort to it, as when they affect the ear, mouth, vagina, uterus, or skin. He suggests its employment in hypertrophy of the heart, in consequence of the remarkable power it possesses of diminishing the action of this organ.

ART. 37.—*On some of the more uncommon results of chronic Lead-poisoning.*

By Dr. H. FEARNSIDE, late Physician to the Preston Dispensary.

(Liverpool Medico-Chir. Journal, July, 1848.)

The following cases exemplify the influence of lead upon the nervous system. The comparative rarity of this form of lead-poisoning, the facility with which it may be confounded with other and very different conditions, and the importance of an early recognition of its cause, combine to render it a subject of very considerable interest.

In an outskirt of the town of Preston there lived, about two years ago, a small family, consisting of a middle-aged man, his wife, and a female dependant. Although the water-supply of the district was abundant and excellent, the house which they inhabited was only provided with rain-water, which was collected in a leaden cistern. This arrange-

ment was adopted from economical motives by the occupier, who was also the owner of the house, and a man of very penurious habits. For several months the water was in constant use for every purpose, without the production of any marked effects. The man's wife, a delicate woman of about forty-five years of age, was the first to become indisposed. She suffered from gradually increasing debility and loss of flesh, with disturbance of the digestive functions and failure of the appetite. But as concurrently with the appearance of these symptoms the catamenia ceased, she paid but little attention to them for some time, thinking them incidental to her age. After their existence, however, for several weeks, she applied for medical advice. She then complained, in addition, of slight feverishness, cough, and pains in the limbs, especially in the muscular parts of the thighs. After the persistence of this state for some time, the muscles of the arms became painful. On a sudden, the complaint left the extremities of the body, and seemed to concentrate itself upon the abdomen, in the form of a violent attack of colic. On the declension of this, she began to suffer from severe pain in the head, especially in the occipital region; and shortly afterwards the nervous system became decidedly implicated. The cerebral functions were strangely disturbed. Various hallucinations showed themselves. Erroneous ideas were formed as to the identity of herself and those around her, and the situation in which she was placed. A mild, but easily controlled form of delirium arose. For some days, this alternated with a different state; if she attempted to speak to those around her, she seemed to have difficulty in finding language, and before she could succeed in conveying her meaning, the idea fled, and a jumble of unconnected words was all that escaped her. And now the condition of the nervous system became one of apathy. Impressions were made upon the senses with difficulty, and speedily vanished; her attention was no sooner gained than it was lost; and she remained for hours at once in a state of semi-consciousness. At this stage, phenomena were occasionally present, approximating to those described as characteristic of catalepsy and ecstasy. On placing the arms, for example, extended in any particular position, they remained fixed in it for some minutes, and then slowly relaxed and sunk down; at other times, she would remain for a considerable period, unconscious of everything around her, but with her eyes fixed, and gazing intently before her. On one occasion, after the action of a purgative, she was for about twelve hours quite insensible. She lived for five weeks after the appearance of the cerebral symptoms, and for four months from the time when her health began to break down. Before her death, she was seized with convulsions of an epileptiform character, which continued with short intervals for forty-eight hours, and upon the cessation of which she passed into a state of complete coma, and died.

The body was examined thirty hours after death. There was great emaciation; the muscles of both arms and legs were much wasted. The head only was opened. The pia-mater was somewhat injected, but there was no thickening nor opacity. The brain generally was softer than natural. Both gray and white portions were somewhat modified in colour, having acquired a dirty pale yellowish hue. No other morbid appearances were perceptible.

Some time before the death of the patient whose case has just been narrated, her husband and niece gave evidence of being under the influence of the same poisonous agent from which she had suffered. In both, repeated attacks of colic were produced. In her niece, a robust young woman of twenty-five years, no other symptoms occurred; but in her husband, a man of fifty, the complaint displayed itself in the muscles of the limbs and in the nervous system. As the colic subsided, he began to experience severe pain in his arms and legs. The muscles moving the arms upon the shoulders gradually failed, and eventually became almost powerless. For some time also he suffered from severe pain in the lower part of the left side of the chest—apparently seated in the intercostal nerves. As the affection developed itself, it assumed more of the character of an acute disease than in his wife. The brain became involved; violent delirium set in, and continued for some days. As the delirium declined, the mind did not recover its clearness and integrity; on the contrary, his sensations and perceptions were vitiated and erroneous. His sense of taste was perverted, so that he no longer recognised ordinary articles of food. Various morbid ideas took possession of him, and some of them remained, and continued to manifest themselves for many months after his apparent restoration to health. His recovery was very slow, and for several weeks he oscillated between life and death. In all three instances the blue line of the gums around the teeth was well marked.

A few comments upon the cases, an outline of which has just been given, may not be without interest.

In the first of them, the absence, for a considerable time, of any colic, and the gradual and insidious manner in which the poisoning took place, led at the outset to a misapprehension of its nature. The cause, indeed, is one which may readily escape notice; for in most large towns, the supply of water, if not unexceptionable, is at all events so free from any noxious matter, that it rarely enters into our consideration as a source of disease. And when water contaminated with lead is habitually drunk, its effects may be such that some care is requisite to distinguish them from very different pathological conditions. Lead so introduced into the body may deleteriously influence the nervous and muscular systems directly, without the production of any premonitory colic. Indeed, colic is by no means an invariable precursor of the other forms of disease occasioned by lead. Thus, Tanquerel des Planches (*Traité des Maladies de Plomb*) mentions that in the patients noticed by him, suffering from lead-diseases, out of 102 examples of paralysis, there was no colic in 39 instances. In the cases under review, it was apparent that there existed a faulty state of the blood; but the fugitive muscular pains, the slight feverish disturbance, and the scanty secretions, caused this to be ascribed to the presence of a rheumatic diathesis. It very soon became evident, however, that this impression was an erroneous one, and, on more minute investigation, the true nature of the complaint was revealed.

The peculiar form in which the affection manifested itself is deserving of notice. The occurrence of convulsions as a consequence of lead-poisoning has long been known; but it is not so generally recognised that delirium and coma may result from the same cause. In the case first related,

a condition allied to catalepsy and ecstasy was occasionally present. This also has been noticed by Tanquerel des Planches in two instances.

It is under such circumstances as those present in the cases just given that the simple test of examining the gums of the patient is so important, and affords such valuable information; for it may be safely stated, that the blue colour of the gums around the teeth is never present except in individuals who either are, or have lately been under the influence of lead, and in such it is very rarely absent. But its presence alone will not suffice to determine the nature of the disease, inasmuch as those who are habitually brought into contact with lead are equally liable, with the rest of the community, to other cerebral disorders.

II. SPECIAL QUESTIONS IN MEDICINE.

(A) CONCERNING THE NERVOUS SYSTEM.

ART. 38.—*On Headache.*

By Dr. SYMONDS, Physician to the Bristol Infirmary.

(*Medical Times and Gazette*, May 15, 1858.)

The following statistical memoranda and conclusions are taken from one of the Gulstonian lectures for 1858:

“*Statistical memoranda.*—In order to obtain some numerical data as to the frequency of certain presumed causes of headache, and as hospital registers would not supply information respecting cases which only casually come within their scope, I distributed some circulars of queries, for obtaining replies to which I am much indebted to many of my professional friends. They were queries which were easily answered, and that the answers were for the most part given thoughtfully and carefully, was obvious from the consistency in themselves, and from the general agreement between the different respondents. Though I cannot bring forward any very new results from the answers, yet they afford confirmation to many of the remarks which I have ventured to make upon the disorder under consideration.

“We may first direct our notice to those points which indicate a neurotic susceptibility on the part of the subjects of the disorder.

“Of 90 cases 76 were females. These numbers established more strongly than I should have expected, the fact, which is testified by most of the old writers, that females are more frequent sufferers. If we endeavour to ascertain whether this liability of females bears relation to their sexual functions, we do not find much to support this supposition. Of the 76 females, 40 were single; out of the 76 there were 35 who referred to the periodical health as having been in some way connected with their attacks: very few dated from puberty—but a large number from early life. Of the 22 out of 90 who referred to previous illness, none specify illness referable to pregnancy or parturition. In 11 loss of blood is mentioned, but while some speak of it as artificial, none refer

to menorrhagia, or to the floodings of abortion or childbed. Now, every practitioner must have noticed that women who have suffered from such losses have been very liable to headache, but it is clear from the replies in general that the specially feminine proclivity to headache is independent of these casualties. But we have said that 35 did refer to the periodical health, and the result of my cross-examination in many of these cases brought me to the conviction that the menstrual function only presented occasions of offence, exciting causes as we often call them, but that the earlier antecedent was a constitutional liability not dependent on this function.

“Putting aside the catamenial function, we inquired into the mode of life, and found nothing to lead one to suppose that this was importantly concerned in the production of female liability.

“The predisposition more probably originates in the nervous system; and it is needless here to adduce proofs of what is admitted on all hands, that nervous mobility, irritability, hyperæsthesia, or by whatever other name we choose to designate susceptibility of nervous disorder, is oftener found in the female than in the male subject. It is likely to exist in organisms which evidence a capability of so much fineness and delicacy of perception, united with so much proneness to emotional excitement, and in which also we observe the functions of organic life to be so readily wrought upon by passing states of thought, sensation, and emotion.

“Of the exciting causes, I find that emotional disturbance has the highest number. Out of 90 cases, 53 declared this to be one of the causes of their attacks; 48 also considered that atmospheric states were to be blamed; and 25 specified thunder.

“It would be little better than a platitude to say that it is far more difficult to trace the origin of a predisposition superinduced on the system, than to indicate causes nearer at hand. Accordingly, in these answers, although the questions afforded sufficient opportunities of stating circumstances in the mode of life, occupation, habits, climatic influence, &c., which might have begotten the tendency, yet very little light is thrown on this subject by the answers. And I incline to the belief, that the cause of this tendency lies too far off to be reached by reference to the experience and consciousness of the individual. It lies in the original mould of the nervous system. This conclusion is supported by the fact that 44 had suffered from very early life, and a large proportion of these use the formula ‘as long as they could remember.’ An additional presumption appears from the proof of inheritance of the liability. In 19 cases the mother is mentioned, in 9 the father, and in 12 both parents; in all, 40 give explicit evidence of hereditary predisposition, and a few others mention cases in collateral branches.

“The negative evidence as to *diet* is curious. Out of the 90 cases only 19 blame their diet, and these, if they specify any at all, only mention some article of food which has been at times an apparent and immediate cause of disturbance; of the remainder, 62 deny that diet has anything to do with their attacks; and 9 make no response, or a very indefinite one.

“For these negative results as to diet, there are two obvious reasons: first, that people are very slow to observe the gradual or habitual

influence of any system of diet into which they have fallen by custom or association; and, secondly, that if food does not produce marked disagreement in the first stages of digestion, they are ignorant of what passes in the duodenal and subsequent processes.

"As to the influence of *climate* in augmenting the predisposition, more evidence is given: 39 either say that climate is a matter of indifference, or they make no answer. But 29 seem very clear that they are least liable to attacks of headache in places where the air is dry and bracing; 6 commend cold atmosphere, 6 condemn it; 8 praise warm atmosphere, 3 dislike it; 6 are in favour of sea-air, and 4 are averse to it. I do not attach any great importance to the paucity of answers as to marine air, because it was not particularised in the questions. Still, if sea-air, *quâ* sea-air, had made any great impression, either as a friendly or as a hostile agency, I think it would have come out in the answers.

"*Fatigue* is mentioned as an exciting cause by 32, though, from inadvertence, the queries contained no pointed reference to it.

"Some of the questions were framed with the view of ascertaining how far numerical statements would bear out the prevalent notion of a connexion between headache and disorders of the digestive organs.

"Out of the 90 cases, 33 said that *vomiting* or nausea occurred in their attacks; 12 reported *nausea only*; 35 experienced neither; 10 made indefinite answers, or none. Of the 45 cases of vomiting and nausea, 33 declared that the vomiting came on after the accession of pain. Of the rest, 6 spoke of the sickness as simultaneous, and 6 gave no evidence on the subject.

"To the question whether the pain had been relieved or removed by sickness; 18 of the 33 said they had been relieved sometimes or partially, and 2 said the pain had given way after prolonged vomiting. Of these one used the expression, 'after bringing up pancreatic juice.'

"These answers would scarcely lead to the inference that the headache depended on the presence of anything in the stomach; for though 18 of those who had vomited spoke of being relieved, this term is very vague, and it is all but impossible to separate the mere effects of the act of vomiting on the circulation and the nervous system, from what might be due to the ejection of food or morbid secretions from the stomach.

"Not more satisfactory are the replies to the question as to the action of the bowels. Of the 90 only 12 distinctly affirmed any relation between their headaches and the action of the bowels; 54 denied any connexion; 17 gave ambiguous answers, and 7 were silent.

"And in reply to whether they had derived relief from aperients, only 15 gave distinctly affirmative answers; 17 said 'sometimes,' or used other qualifications; 6 were doubtful; 3 were certain that they were made worse by aperients; 36 declared that they were not relieved, and 13 gave no distinct response. These replies do not tell much either way. When aperients have been taken the pain generally subsides spontaneously before the bowels have acted; and, therefore, even with all the disposition which most patients manifest to attach undue importance to this function, they would not assign the relief to the action if the action occurred subsequently to the relief of pain. And yet we

know theoretically that they might do so, since the artificial impulse given to the function of the small intestine may have removed sources of irritation. On the other hand, the affirmative respondents are as likely to have unconsciously set their replies into consonance with their preconceived notions of the value of aperients.

"To have asked my catechumens whether their headaches had anything to do with bile or bilious disturbance would have, I well knew, called forth a chorus of 'Yes.' But as I should have been none the wiser, I thought it better to limit the question to the 'accumulation of bile manifested by the skin.'

"Of the 90, 65 were obliged to say 'No;' 14 gave a doubtful or qualified 'Yes;' and 11 were mute. When asked if they were liable to indigestion, 39 answered in the affirmative, but with qualifying terms, indicating that they were not quite certain about the matter.

"I will not trouble you with further details, but as far as these replies bear upon the relation between headache and disordered digestion, the evidence is, to say the least, ambiguous and unsatisfactory. But if any decided inference is to be drawn from it, we must conclude rather against the existence of such a connexion in many of these cases, and we do not advance beyond the fact that when there is the disposition to cephalalgia, the digestive organs may afford occasions of excitement to that disposition.

"Two of the questions were intended to elicit facts as to the dependence of the attacks on hyperæmia. Of the 90 cases, 38 reported that their headaches were increased by the recumbent position, 27 that they were relieved, 5 were doubtful, and to 17 it was a matter of indifference. Of those who were relieved, some said that the position at first was painful, but afterwards that it became comfortable; and others appeared to think that the rest was quite as important an item as the recumbence. Change of posture, of course involving motion, almost uniformly aggravates the pain; and we may fairly suspect, that of those who say that the pain is increased by laying the head low, some have been deterred from fairly trying it by the first discomfort of the change.

"To the question whether the pain was relieved by stimulants, 35 answered in the affirmative, 30 simply denied, 8 reported it to be absolutely increased, and the remainder were doubtful or indifferent, or had never tried such remedies. I do not think that much value can be attached to these answers. A headache may be associated with passive congestion, and may be relieved by stimulants. It may be purely nervous, and yet aggravated by them, because in so tender a state of the nerves the slightest increase of vascular disturbance will increase the suffering, whether produced by exciting drinks, or as we have just noticed, by slight changes of posture.

"The inquiries as to the operation of these and other agents were made, chiefly to obtain information as to the pathology and etiology of the disorder. But one general question was put as to therapeutics: 'From what kind of remedies have you derived most relief?' The question was too wide; for, while some enumerate the remedies which have done good at the time of pain, and also those which have lessened the frequency of returns, others have confined themselves to the treatment

which has relieved the attack: 26 specify benefit from *tonics*, *quinine* being mentioned oftener than any other; 15 mention *sedatives*; 11 insist on profound *quiet*; 4 speak well of *cold applications*, 2 of *warm*; 1 ascribes relief to *leeching*, and 1 (the same respondent) to *blisters*; 3 are so unenterprising as to have tried nothing; 12 have been altogether unsuccessful, and they now rely only on time and patience. The rest give vague answers, not requiring further comment.

"As far, then, as the information is positive, we may say that the evidence is in favour of tonics and sedatives.

"It is curious that, in the whole series no mention is made of phlebotomy or cupping; and leeches are noticed only by one person. Had the same set of questions been answered five-and-twenty years ago, I presume that antiphlogistic remedies would have received more notice.

"Before closing this statistical attempt, I may mention that of the 90 cases, 43 report that the attack comes on very early in the day—most of them on *waking*. It would be easy to speculate on the cause of this general fact. Some would blame the position in bed; others the long interval after food had been taken; others the state of the nervous centres belonging to sleep; others the diminution in the intensity of terrestrial magnetism in the morning. For my own part, I wait for further illumination on this as on many other topics belonging to the whole subject."

The conclusions to which Dr. Symonds arrives are these:

1. That headache has an importance of its own; and that, whatever connexion there may be between this affection and the disorders of other parts of the body, the head itself is to have the chief consideration.

2. That headache, whether primary or sympathetic, considering the frequency of its occurrence, is rarely dependent on disorder involving danger, or serious impairment of the function of the brain.

3. That symptomatic headache, even when belonging to grave lesions, may be benefited by the treatment applied to the primary forms.

4. That the primary headache, whether caused by an original susceptibility in the ganglionic nerves of the brain or by a morbid sensibility resulting from previous disease or weakness, is to be relieved by sedatives, and to be cured by tonics and nervines. He will be the most successful practitioner who can best wield those two great weapons, quinine and arsenic.

5. There is a natural tendency in this disorder to decline with advancing life.

ART. 39.—*On the treatment of Delirium Tremens.* By Dr. LAYCOCK, Professor of the Practice of Medicine in the University of Edinburgh.

(*Edinburgh Medical Journal*, Oct., 1858.)

The general rules for treatment of delirium tremens are simple:

1. The patient should be placed in as complete a state of muscular repose as possible. To this end, he should, if practicable, be kept in bed. Muscular activity necessarily exhausts the nervous system; hence, quiet of the muscular system facilitates repair of nervous energy. If, however, the patient cannot be kept in bed without mechanical

restraint, it is on all hands allowed to be better not to restrain him, as the waste of motor power is much greater in the continued attempts the patient makes to keep from restraint, than in his usually quiet wanderings after unmeaning objects. Should his delirium be of the violent kind, absolutely needing restraint, chloroform would, perhaps, be in general a safer remedy than the strait-waistcoat, although not a remedy to be administered without serious consideration as to its fitness in each case. 2. All *sensational* stimuli should be removed, and all emotions, agitating thoughts, or anxieties, be prevented. 3. Food of a suitable kind should be carefully given from time to time; no alcoholic stimuli of any kind administered as articles of diet, unless specially indicated. 4. Where there is a tendency to diaphoresis, it should be encouraged as an elementary process. 5. The surface, and especially the feet, should be kept comfortably warm. If the head be hot, the hair may be cut short, and a gentle douche, for one or two minutes, applied every three or four hours; this is rarely necessary, however, much less shaving the scalp. 6. An experienced nurse must attend the patient. But, above all, it is essential that the practitioner be clear in his etiological diagnosis, so that he may be knowing as to the powers of nature. He should first determine whether the patient be under the influence of alcohol or not, and ascertain clearly whether there be any important complications. If the patient be alcoholized, and no important complications be discovered, he may consider the sleeplessness and delirium as of no great pathological importance, and calmly and confidently wait the result of a few days' judicious watching and general management of the case.

Dr. Laycock then examines some of the disputed points in the pathology and treatment of the disease.

1. *Is the delirium due to the withdrawal of alcoholic stimuli?* If this be not the fact, then the administration of spirits as a remedy is very questionable, if not wholly unjustifiable. Now, in only one of the twenty-two cases admitted into the Infirmary last summer, was there a shadow of ground for stating this; in each of the twenty-one the patient was alcoholized when admitted. In the twenty-second case (poisoning by opium), the grounds for the assertion were of the slightest; other far more probable causes of the delirium were in operation than the return to sobriety. I cannot recollect, in fact, any case in which I could attribute the delirium to a withdrawal of the liquor. I regret to find that I differ with so high an authority as Dr. Watson on this point. Dr. W. says ('Lectures on Practice of Physic,' 4th edition), "In a large majority of instances you will find that he has been an habitual drunkard; and very frequently that, from some cause or other, his habitual stimulus has been diminished or taken away." But then Dr. Watson goes on to qualify this statement, by adding, "Some accidental illness has befallen him, and he has been restricted to low diet; and, as a sailor would say, 'his grog has been stopped.'" So that, in fact, Dr. W. includes "illness" and "low diet" (very common exciting causes) with the stoppage of the grog. It would be interesting to have Dr. Watson's experience on that one point alone,—I am much inclined to think it would correspond with my own. Dr. Wood, of Philadelphia, in his 'Practice of Medicine,' makes this disputed point a part of his definition

of the disease. "This is the delirious affection," he says, "which follows the suspension of the habitual use of alcoholic drinks. Its essential character consists in the cerebral debility consequent upon the cessation of an accustomed excitement." Then, in describing the symptoms, course, &c., he goes on with, "the first effects of the suspension of the stimulus are feelings of great weakness." . . . "Should circumstances, however, prolong the abstinence or privation, this preliminary condition becomes aggravated, and the disease is fully formed," &c. Now, all this is by no means true to nature, as all observers must acknowledge; the more the man drinks the worse he is, until nature forbids more. Here, again, it would be interesting to have the data upon which these assertions are based. It would probably be seen that the delirium has been rather due to some "illness" coincident with the cessation of the stimulant, or to low diet, or to almost entire abstinence from food, or to want of sleep, or overwhelming mental anxiety, or some other *concomitant* causes. I dwell more especially upon this point in the etiology, because it is of vital importance as to the treatment of delirium tremens; and I therefore affirm distinctly and emphatically, that I know of no recorded facts which contradict my experience, *i. e.*, which prove that the affection is consequent, actually or virtually, upon the withdrawal of intoxicating drinks. Now, on the other side of the question, the evidence is abundant. Multitudes of drunkards cease drinking without suffering from the disease, or from any approach to it. Dr. Craigie, in his very able essay on the affection ('Elements of the Practice of Physic'), says, that he never witnessed an instance of this mode of development; and after perusing all the published cases extant, he could not perceive that any of them, excepting one recorded by Dr. Armstrong, afforded satisfactory evidence that the disease is induced in consequence of the sudden abstraction of spirituous liquors. Even that case, he adds, may be explained on other grounds. Dr. Craigie further states, that none of the Continental physicians who have written on the subject, support the doctrine; while Dr. Ware, of Boston, U.S., could not detect any such etiology in a large proportion of cases he examined. On the other hand, it is found, that in the recorded cases, as in those I treated, the symptoms were most usually developed after a continued fit of drinking for several days, during which the blood had become charged with the liquor. I would finally refer to Dr. Alexander Peddie's valuable essay on the affection, as containing additional and most conclusive evidence on this point. It may, therefore, be now stated, as a fact beyond question, that the delirium of the drunkard is due to other causes than cessation from the use of alcoholic stimuli.

What, then, are the causes of the affection? Clearly, I think, not one operating singly or specially, but several acting together,—what are termed by Sir William Hamilton con-causes. I have already indicated several of these. It is a subject, however, I need not dwell upon, as most systematic writers recognise the resemblance between the delirium attacking the drunkard, and that attacking temperate persons under certain conditions of the brain, the blood, and important organs; what is certain is, that drunken or sottish habits strongly predispose to the affection when those conditions arise. The etiology, therefore, of de-

lirium tremens comes under the general etiology of forms of delirium not due to structural or inflammatory disease within the cranium, and is a question of medical psychology.

Is opium necessary to the cure of delirium tremens? Dr. Watson says ('Lectures on Practice of Physic,' 4th edition, p. 410), "The great remedy in delirium tremens is *sleep*; our most powerful means of inducing sleep are to be found in opium. The opium must be given in full doses; and it must be fearlessly repeated, if its desired effect do not soon follow. If the patients pass many nights without sleep, they will die. . . . After clearing out the bowels by a moderate purgative, you may give three grains of solid opium; and if the patient show no inclination to sleep after two or three hours have elapsed, you may begin to give one grain every hour till he does sleep." So, again, Dr. Wood, of Philadelphia (as deservedly high an authority in his own country as Dr. Watson is in this), describing the plan he follows, remarks: "Two grains of opium, half a grain of sulphate of morphia, or an equivalent quantity of one of the liquid preparations of the drug, are given every two hours, and steadily persevered in until sleep takes place, or a decided narcotic impression is evinced." How contrary all this is to my experience in the Royal Infirmary, is obvious. I will only here add, that I have lately seen three cases, in consultation, which were being treated after the method recommended by these eminent physicians, with no benefit. In two of these (very severe cases), my medical friends approved of my suggestion, that the administration of opium be suspended, and a simple treatment followed; and the results were most satisfactory. In the third, it was thought advisable to persist in the stimulant treatment, and the case finally ended fatally. The evidence in favour of the stimulant method, by opium, wine, &c., is, in fact, one-sided; those who adopt that method do not try the other. But experience abundantly shows that sleep, and therewith return to health, will come on naturally in delirium tremens without the use of any narcotics, or even any drugs whatever. Continued sleeplessness, although highly predisponent to morbid action, does not necessarily destroy a man. I know a gentleman who never slept a moment for a month, yet with no bad results. It is the causal and concomitant conditions of the sleeplessness which are to be dreaded. Hence, when sleep is induced by opium, and these conditions remain, the patient dies nevertheless. When sleep supervenes naturally, the causes of the delirium and sleeplessness are alike ceasing. The spontaneous termination of the disease in sleep is well known. Many years ago, Dr. Kuhn found no measure to answer so well as putting the patient into a dark cell, and "leaving the disease to work itself off spontaneously." Seamen, just after leaving the shore, are apt to suffer an attack, and have usually no treatment, yet recover after sleep. Esquirol and Calmeil treated their cases by the expectant method, using no narcotics, only diluents and warm baths, with repose; in the large majority of cases, recovery took place in four or five days. Dr. Ware, of Boston, watched and described the disease as it ran its natural course uninfluenced by remedies, and found that it was a self-limited affection; dating from the time when the state of entire watchfulness and delirium commences, he found that it terminates by natural

sleep in not less than sixty, or more than seventy-two hours. The only cases in which death took place after sleep came on, were those which had been treated by large doses of opium. He treated twenty-nine cases on the expectant method, one died; twelve by emetics, one died; eight with opium, four died. Opium, then, is not necessary to the cure of delirium tremens.

Is opium a safe drug to administer freely in delirium tremens? It is a remarkable illustration of the influence which usage and routine exercise on the judgment, to find how unsuspectingly, as to its bad effects, opium is prescribed in this class of affections. Patients who sleep and recover after its administration, sleep and recover, it is said, *propter hoc*. If they do not sleep, they have not had enough; but if they sleep and die comatose, with livid face and contracted pupils, then the disease, and not the drug, proves fatal. Here, I think, is a whole string of fallacies. Experience, rather than theory, is the safest guide; and that assures us it is never, under any circumstances, a safe proceeding to administer from five to ten or fifteen grains of opium in as many hours. Experience also shows, that in delirium tremens, while many have recovered without opium, and some in spite of it, none can be said to have died for the want of it. And if we were to inquire theoretically in what class of cases opium should not, or need not be given, we should find very few left in which it should. It is clear, that in the simple, uncomplicated, alcoholized cases, in which spontaneous recovery under judicious management is certain, opium is not needed, and is at least as dangerous, in the large doses recommended, as in the healthy state. Is it admissible in the complicated cases, as those with masked gout, cirrhosis of the liver, or Bright's disease—or with gastritis, bronchitis, pneumonia, pericarditis, meningitis—or in those labouring under some epidemic, as scarlatina, influenza, continued fevers—or with meningitis, or cerebral inflammation from mechanical injuries? In any of these it would be a dangerous practice indeed to give opium, in the large doses recommended by Dr. Watson and Dr. Wood. If, then, we exclude all the uncomplicated cases, and those thus complicated, how many remain for the heroic doses?

Is it necessary or safe to treat delirium tremens with alcoholic stimuli? If the patient be alcoholized at the very moment when it is necessary to prescribe for him, and is manifestly suffering from the effects of the alcohol, it seems hardly rational to propose that more should be given him "every two or three hours." Doubtless, people recover in spite of further doses—mainly, I think, because it is administered in diminished quantity: they may even recover after the administration of both alcoholic stimuli and opium; but it is to be feared that they sometimes sink under the remedies. If I am rightly informed, the stimulant plan, with alcoholic drinks and opium, has been usually followed at the Royal Infirmary, except from 1839 to 1842, with the following results:

TERMINATION OF CASES OF DELIRIUM TREMENS, TREATED IN THE
ROYAL INFIRMARY, EDINBURGH.

	Males.		Females.		Total per cent. of deaths.
	Cases.	Deaths.	Cases.	Deaths.	
1st July, 1839, to 1st October, 1841, }	64	7	11	1	10·6
1st October, 1841, to 1st October, 1842, }	32	5	4	0	14·0
1st October, 1842, to 1st July, 1843, }	32	5	8	2	17·5
1st July, 1843, to 30th September, 1844, }	26	11	8	3	41·0
1st October, 1844, to 30th September, 1845, }	29	12	9		36·8
1st October, 1845, to 30th September, 1846, }	50	16	10	2	30·0
1st October, 1846, to 30th September, 1847, }	61	23	9	2	35·7
1st October, 1849, to 30th September, 1850, }	46	10	4	0	20·0
Totals,	340	89	63	12	25·0

So that, of 403 cases of both sexes treated in the Royal Infirmary during $8\frac{1}{4}$ years, 101 died, or 25 per cent.; of males only, the deaths were over 26 per cent. As a contrast to this, I give the following: Admitted into the Infirmary, under my care, 15 males, 9 females—total, 24; deaths 0. In private practice, 4 males; death 1 (under opium and alcoholic stimuli). Dr. Peddie treated “upwards of 80 cases in males, many of them very severe ones,” without opium, “with uniform success,” *i. e.*, without a death. That the Infirmary mortality of past years is greatly in excess, for some special reasons, is obvious from other data. Dr. Rogers reports, that of 75 cases admitted into St. George’s Hospital during five years ending 1855, 14·6 per cent died. The mortality in the General Hospital at Calcutta from 1848 to 1852, and in the Medical College Hospital, 1851-1853, was 15 per cent. in 321 cases. In his report on the health of the army (1853), Colonel Tulloch gives the mortality of the cases of delirium tremens in Great Britain at 17·6 for the infantry, and 13·8 for the cavalry. Of writers, Calmeil states the mortality at 5 per cent. (but he treated expectantly); Bougard at 19 per cent., and this is the highest I know of, yet it is much below that in the Infirmary. If, however, we take the cases admitted between 1st July, 1843, and 30th September, 1847 (Dr. Bennett’s Statistics), we find that 62 male patients died out of 166, or $37\frac{1}{2}$ per cent.,—an enormous mortality, and only paralleled by that of the Glasgow Infirmary. From 1842 to 1848, there were 35 cases admitted to that institution, and 17 died, or nearly 50 per cent. I have no details as to the method of treatment followed; but I believe that Dr. Peddie is correct in

saying, "The common practice [in Scotland] has been, and, I have reason to know, in many quarters still is, to give from one table-spoonful to a wine-glass of spirits every two or three hours, either alone or combined with opium." I think it will be also admitted that such is the general practice elsewhere; and, consequently, that the other statistics given above may be taken as indicating the results of treatment by opium and alcoholic stimuli. The great disparity between the *lowest* death-ratio of these, and the results of the expectant method, is so great, that it cannot fail to arrest the deep attention of the thoughtful practitioner.

Is the treatment by tartar emetic and vomits necessary? A simple emetic will be useful, if there be bile or acrid stuff in the stomach. Dr. Klapp, of Philadelphia, observing that the disease was apt to occur upon the cessation of an attack of vomiting in the intemperate, and that spontaneous vomiting relieved it, theorised thereon and came to the conclusion that it was judicious to give two grains of tartar emetic every fifteen minutes until it operated as an emetic. The stomach and nervous system being often half paralysed sensorially, large quantities (more than gr. xvj) were sometimes found to be necessary to induce vomiting! It is not possible to esteem this as other than a most dangerous proceeding. Dr. Graves administered tartar emetic more carefully, with the view of combating vascular excitement, and combining it with small doses of opium. In that particular class of cases in which the delirium is more sthenic than asthenic, and especially in those in which the blood is not alcoholized, this combination has been found beneficial; it is often thus used in cases of recurrent maniacal delirium. Dr. Peddie had arrived at the use of tartar emetic before he knew of Dr. Graves' views, or that Stoll, Göden, Klapp, and others, had recommended it. He gives it in doses of gr. $\frac{1}{4}$ to gr. $\frac{1}{2}$, in simple solution, every two hours, believing it exercises a "direct influence in reducing the vascular excitement of the brain, soothing the nervous system, and diminishing muscular power; and its more direct action is exerted on the functions of the skin, kidneys, and intestinal canal." Although I feel some doubts as to this theory of its action, I can well understand that tartar emetic, judiciously administered in the way Dr. Peddie recommends, may be of service for the alleviation of the symptoms in some of the more violent cases. Nevertheless, in the mild forms of the disease—and these constitute by far the most numerous—I should prefer to wait upon nature, and treat the symptoms, as they arise in each individual case, according to the general rules of art. Delirium tremens, like all affections of the brain, is little amenable to remedies if associated with important cerebral disease, so that a few doses of tartar emetic, more or less, are of little importance in that class; while, when not so associated, or not complicated with important visceral disease, it happily soon terminates in health, if nature be left to do her work.

The practice of bloodletting in delirium tremens, and the administration of large doses of calomel, digitalis, and other active drugs, are sanctioned by so few systematic writers, that it is unnecessary to examine into their worth. It is to be hoped that the use of other heroic remedies will equally pass out of use, and a method of treatment be followed, grounded upon an accurate knowledge of the natural history and course

of the affection in each case, to the exclusion of false theories founded on imperfect observations.

ART. 40.—*The distribution of the mortality from Hydrophobia in England.*

By MR. J. R. RADCLIFFE.

(*Medical Times and Gazette*, July 3, 1858.)

Mr. Radcliffe commences this paper (which is intended to illustrate certain peculiarities in the mode of extension and prevalence of epidemic diseases, and to offer suggestions for the better observance of such diseases) by stating, that the sources from which he had obtained the particulars that he was about to lay before the Society were the Bills of Mortality for the Metropolis and the Annual Reports of the Registrar-General. The Bills of Mortality for London were commenced, as a regular series, in 1603, but no death from hydrophobia is recorded in them until the year 1728. In the annual bill of that year is the entry, "Died from the bite of a mad dog." The term "hydrophobia" was first made use of in the Bills of Mortality in 1819. The absence of any record of death from so well-marked a disease as hydrophobia in the Bills of Mortality, for a period of 125 years subsequent to 1603, is a fact of considerable interest, and the great rarity of the disease, if not its entire absence from the metropolis during that period, may be assumed, notwithstanding the imperfect character of the mortality records of the period. After 1728, deaths from hydrophobia were frequently recorded in the yearly bills, although intervals of one, two, three, or more years, when no deaths were returned, are not uncommon; but in no instance does the number of deaths from the disease entered in a yearly bill exceed 4 until 1824, when 7 deaths occurred. After this year intervals where no deaths happened are rare; and in 1825, 4 deaths were recorded; in 1826, 4; in 1827, 1; in 1828, 2. The bills for 1829 and 1830 are wanting. In 1831, 6 deaths were returned; in 1832, 3; in 1833, 4; in 1834, 8; in 1835, 0; in 1836, 1; in 1837, when the Registrar-General's returns commenced, 7 deaths occurred in the latter half of the year alone; and in 1838, 12 deaths happened. After 1838, the number of deaths annually did not exceed 4, until 1854, in which year 9 deaths from the disease occurred. The Registrar-General's returns date only from July 1st, 1837. Brief as is the period over which the returns extend, an hiatus of five years occurs from 1843 to 1847 inclusive, during which period no account of the causes of death in the different registration districts is given. So far as the returns are available, they show considerable variations in the amount of mortality from hydrophobia in the whole of the registrative districts. Mr. Radcliffe having completed his examination of the records of mortality from hydrophobia in England, proceeded to direct more particular attention to the following points: (*a*) The absence of any record of death from hydrophobia in the London bills of mortality for a period of 125 years previous to 1728, and the almost constant occurrence, year by year, of deaths from that disease since that period. He regarded those facts as being of considerable interest; and suggested that they might be regarded as indicating a periodical change, the intervals of

which are of long duration. (*b*) The marked increase in the amount of mortality from hydrophobia in London since the commencement of the present century, is well worthy of note. This increase was contemporaneous with an unusual prevalence of epizootic hydrophobia. (See 'Maine's Canine Pathology,' p. 99.) On the other hand, Dr. Layard writes ('Essay on the Bite of a Mad Dog,' London, 1763, p. 120), that there was a general alarm of canine madness in London, Westminster, and many parts of England in 1760. No indication of an unusual prevalence of epizootic hydrophobia in London and Westminster is to be found in the bills of mortality for that year; indeed, from 1760 to 1766 inclusive, no death from hydrophobia was recorded in the metropolis. (*c*) The variations in degree of prevalence of hydrophobia in London from the commencement of the bills of mortality to the present time, and as judged of by the amount of mortality, may be regarded as indicia of periodical phenomena; thus considered, there would seem to exist, with regard to the disease, a law of greater variation, the periods of increment and decrement of which occupy an indeterminate cycle of many years; and a law of lesser variation, the periods of which of comparatively short duration are manifested during the period of increment of the greater variation. As to the period of increment of the greater variation commencing in 1703, it presents many slight variations until the beginning of the present century, when the degree of intensity rapidly increased until the year 1838. But it remained to be seen whether the culminating point was reached in that year, or whether the maximum has still to be attained. (*d*) The apparent gradual spread of hydrophobia from district to district over a great portion of the kingdom during the whole period observed in the Registrar-General's reports, is a fact of considerable interest. Mr. Radcliffe advanced reasons for the belief that the theories usually entertained to account for the extension of hydrophobia are not capable of explaining satisfactorily the whole of the phenomena accompanying the spread of the disease. He contended that the data we possess are insufficient for the solution of the question; and he directed particular attention to the very dubious character of the conclusions which might be derived from the records of the mortality from hydrophobia. He argued that under these circumstances the proper course to pursue was to frame a systematic method of observations by which a sufficient number of well-recorded facts could be accumulated to form a sound basis for reasoning. He remarked that a system of observation by which the phenomena of disease could be registered on one plan—a plan which would admit of a comparison of observations taken in different localities—is a desideratum in medical science; and he suggested that such a system of observations as applied to epidemics should emanate from the Epidemiological Society.

ART. 41.—*Case of Spontaneous Hydrophobia.* By M. HENRICH.

(*Henke's Zeitsch.*, 1858; and *Gaz. Hebdom. de Méd. et Chir.*, Oct., 1858.)

CASE.—F. K—, æt. 30, on the 29th of May, 1851, began to suffer from headache extending over the whole scalp, and symptoms of catarrh. The day following, in the morning, there were rigors and severe shuddering. In the

evening of the same day, called in haste to the bedside, M. Henrich found the patient sitting up in bed, the countenance drenched in sweat, pale and expressive of terror, the eyes injected, brilliant, haggard, the voice indistinct and interrupted. The patient, also, complained of pains, and a sense of constriction in the throat and chest, with intense thirst, and without the power to drink. Then the sense of constriction became greatly aggravated, and the patient seemed to be in danger of dying from suffocation. Saliva flowed in considerable quantity from the mouth. Pulse 90, soft. Throat reddish, and covered with tenacious mucus.

An attempt to drink, which was greatly resisted, brought on an attack of suffocation, and the cup was thrown convulsively away. Light, or a current of air, had no effect in bringing on the spasm, and there were no vesicles of Marochetti under the tongue.

The day following all the symptoms were aggravated (he had been bled and blistered upon the chest in the meantime); the spasms became general, and of a tetanic character, and death followed after two or three attacks of opisthotonos, the mind remaining quite clear up to the end.

The only appearance after death which was at all unnatural was a slight swelling of the base of the tongue.

M. Henrich, after the most careful examination, could find no evidence of any bite by a mad dog, and the only additional fact which remains to be mentioned is, that the man had been very much depressed in spirits, and very much given to intemperance in sexual matters.

ART. 42.—*On the treatment of Chorea by large doses of Tartar Emetic.*

By MM. BOULEY, GILLETTE, and BONFILS.

(*Archiv. Gén. de Méd.*, July, 1858.)

Rasori's plan of treating chorea by large doses of tartar emetic has been recently tried in some of the Parisian hospitals, and the report says with much success. The plan of M. Gillette is to administer the drug in series of three days, and leave the patient at rest for three days between each series. The object is to obtain *tolerance*, and in order to this the dose is progressively increased, twenty, twenty-five, or thirty centigrammes for the first day, double this quantity for the second day, triple this quantity for the third day. In the next series of three days the former dose is augmented, and so also in the following series, if it is necessary to prolong the treatment. The plan of M. Bouley, on the contrary, is not to produce tolerance, but to develop quickly the physiological effects of the drug—nausea, vomiting, purging, and so on.

M. Bonfils, who was an *interne* at the Hôpital des Enfants, who carried out M. Gillette's plan under that physician, and who makes the subject the theme of his inaugural thesis, says that this plan may be carried out without danger, and that it cures the disorder very rapidly. In ten cases, he tells us, nine were cured in a mean period of sixteen days. Only one case proved refractory, and in this there was reason to suspect some organic cerebral disease. The more grave the disorder, the greater, according to M. Bonfils, would be his confidence in this mode of treatment.

We must know more of this mode of treatment before we can endorse these favorable statements.

ART. 43.—*On Local Narcotic Injections in Neuralgia.*

By Mr. CHARLES HUNTER.

(Medical Times and Gazette, Oct. 30, 1858.)

In this communication Mr. Hunter brings forward two cases of neuralgia, in which he had tried this mode of treatment. In both cases he finds that considerable relief was obtained—relief which had not been obtained by any previous treatment; that there were certain advantages, as also disadvantages, attending its employment, and that the most serious objection was the production of abscess, because the injection had to be continued some days.

Abscess having resulted in both these cases, Mr. Hunter has given up the localization of the remedy as proposed by Dr. Wood, viz. the injecting the narcotic into the most painful spot affected by neuralgia, or the part which if pressed upon would occasion a paroxysm, and has adopted the employment of narcotic injections into the cellular tissue of various parts of the body.

As the abscess in one of his cases prevented the author from injecting the neuralgic part, this mode of treatment was desisted from for eighteen days, during which time doses equivalent to those which had been injected were given by the stomach.

It is remarkable that no relief followed this mode of administering the morphia; on the contrary, although it was regularly continued, the pains became as sharp and as frequent as they were before the local injection had been tried; and his sleep was again “only in half hours,” instead of several hours’ duration.

At the end of eighteen days, all general treatment being left off, Mr. Hunter once more commenced narcotic injections of the cellular tissue, not this time of the part affected with tic, but of healthy parts, such as the neck, arm, &c., and taking care that the same spot should never twice be subject to injection.

Since beginning this plan the man has again received remarkable benefit. The paroxysms during the day are shorter, the countenance of the man is far quieter, and he sleeps from seven to eight hours every night, and he now has these advantages without the risk of abscess.

With regard to the other case, the author ceased the localization of the narcotic on October 6th, on account of the abscess; but continued the injection ever since, inserting the point of the aneurismal syringe at various places in the integument of the arms. About three weeks have now elapsed since he began to vary the site for injection, and no sign of abscess at any one point has shown itself, nor indeed of inflammation.

In conclusion he draws attention to these points:

1. That by the injection of the narcotic into the cellular tissue of a part distant from that affected with the neuralgia, the relief that follows appears quite as great as when the injection is into the cellular tissue of the neuralgic part.

2. That therefore the idea that the relief results from the localization of the remedy in the painful part is erroneous—equal

relief being afforded in either case (injected into the painful part or elsewhere).

3. That with the abolition of localization of the remedy the great objection to narcotic injection is done away with, because no inflammation or abscess follows a single narcotic injection.

4. That the advantages of this mode of administration still remain such as—

(a) The effect of the narcotic being more immediately produced (sometimes almost instantaneously).

(b) The greater certainty of its effect; the knowledge of the exact amount introduced and absorbed; and the avoidance of contamination or alteration which it is liable to administered by the stomach.

(c) The avoidance of constitutional irritation, sickness, headache, &c.

ART. 44.—*On the relation of common and tactile sensibility in disease.*

By Dr. SIEVEKING, Physician to St. Mary's Hospital, &c.

(*Medico-Chir. Rev.*, Oct., 1858.)

“The current teaching of physiologists has hitherto been, that the faculty by which we perceive painful sensations, and the faculty which enables us to distinguish the different characters of surfaces that come into contact with our integuments, are identical; and that the channels of common sensation which convey pain, are the same as the channels of tactile sensibility which convey the impression of touch. Schiff and Brown-Séquard have, however, recently essayed to show the error of these views physiologically. The opinions of these distinguished inquirers do not entirely agree with regard to the channels by which the different impressions are conveyed to the sensorium, but both hold that common sensibility and tactile sensibility manifest themselves to the individual by the aid of different sets of fibres.* I have arrived at a similar conclusion by pathological observation, and in the present condition of our knowledge on the subject, it may be useful to adduce any well-authenticated fact that assists in elucidating the subject. The question, then, at present before me is this: are the different impressions made upon the cutaneous surface differently received at the point of contact? are there distinct organs by which the impressions are conveyed to the organ of consciousness? or, are the media by which the

* Schiff's views are briefly these, that the white posterior column of the cord alone conduct tactile sensation, while common sensation passes by the gray matter, neither portion of the cord co-operating with, or acting for, the other in the performance of the function of sensation. (Moleschott's 'Untersuchungen,' 1858, iv, 1.) On the other hand, Brown-Séquard, whose delicacy in operating it is impossible to see without feeling great confidence in the conclusions which he puts forward in his lectures, maintains, in reference to the present question, that “most of the elements which are employed as conductors of purely tactile impressions seem to pass by the same parts of the spinal cord as those which transmit the impressions which produce pain.” Brown-Séquard holds that section of the posterior columns of the cord induces hyperæsthesia in all parts lying posterior to the divided part, a point that is specially denied by Schiff. For the arguments and experiments upon which Brown-Séquard bases his views, we refer the reader to his lectures which are appearing in the 'Lancet,' and particularly to the fourth. ('Lancet,' July 31, 1858.)

impressions are conveyed identical, the difference in the impression depending upon the interpretation given by the central organ? In the latter case, the inference would be, that there are elective affinities between a portion of the brain for impressions attributable to common sensibility, which are not received by another portion of the same organ which has an affinity for the impressions of touch. The first view appears to me much more consonant with what we know of the working of nature, and of the laws that govern organic mechanism.

"The term, 'common sensibility,' I employ to denote the perception of a more or less painful impression made upon the surface; tactile sensibility may be defined as the power of distinguishing with greater or less facility the inequalities or other characters of surfaces brought into contact with the skin. The simplest way of determining the delicacy of the tactile sensibility of a part is, by employing two points moveable on a graduated beam, and impressing them at the same time upon the part to be tested; the smallest distance at which the person experimented upon can distinguish the two points as distinct impressions is a measure of the tactile sensibility of the part. Until the two points are felt in this way, the impressions seem to be produced either by a single point or by a broad surface. There is no relation between the tactile effect and the amount of pain produced, unless it be an inverse ratio; for if actual pain result from the force with which the points are made to impinge, it would rather overpower than promote tactile insensibility.

"The capability of distinguishing weights has been attributed, and apparently with justice, to a distinct property of the muscles, which has been termed the muscular sense. In this way sensation has already been narrowed. It appears to me that pathological facts that have come under my notice justify our proceeding yet further, and separating from one another common sensibility and tactile sensibility.

"We frequently meet with cases in which the patient expresses himself in terms leading the physician to suppose that the two kinds of sensibility are distinct; but as long as the patient's impressions are only subjective, and incapable of actual measurement or delineation which can be made demonstrable to others, they are likely to be set down among the capricious phenomena of nervous derangement, our ignorance of which we cloak with such terms as hysteria, hypochondriasis, spinal irritation, and the like. In the short notice which I gave of the æsthesiometer, and its mode of application,* I showed, by an instance culled from actual practice, how subjective numbness might coexist with perfect tactile sensibility. As I am disposed to offer a different interpretation for the phenomenon from what I did then, or rather to define the interpretation more precisely, and as it will serve for comparison with the cases I wish now to refer to, I quote it again:

"E. M'M—, æt. 52, suffered for six months before the first consultation from numbness and formication of the left hand, with severe nocturnal pains along the tips of the fingers and at their metacarpal ends; the patient rarely had pain in the thumb, and none in the palm of the hand.

* See a description of Dr. Sieveking's æsthesiometer in the 'British and Foreign Medico-Chirurgical Review,' January, 1858, p. 280. The æsthesiometer may be obtained from Messrs. Elliot, philosophical-instrument makers, 30, West Strand.

There was frequent vertigo. To determine the character of the numbness, the æsthesiometer was applied, and the patient was found to distinguish one tenth of an inch equally well at the tips of the middle and third fingers of both hands; the instrument aided in the determination of the diagnosis by showing that the numbness was purely subjective, and not the result of a true paralytic affection.'

"I have recently had two cases under my care at St. Mary's Hospital, which, each in a different way, prove to my mind that there are in man two distinct functions, and therefore two distinct sets of nerves through which they act, viz.—common sensibility and tactile sensibility; the former being manifested by pain, the latter by the power of distinguishing the character of the surfaces with which a part is in contact. It is manifest that in health the two must apparently be identical, because the painful impression and the impression produced by the character of the impinging body will necessarily be referred to the same spot. In disease, however, this may be otherwise; and if we can satisfactorily show that one of these functions can exist without the other, the legitimate inference is scarcely to be avoided, that they are not identical, and that the media by which they manifest themselves, and through which the individual becomes conscious of them, are also distinct.

"The cases to which I shall refer have many points of interest apart from the question at issue. The space at my disposal will not, however, permit of my doing more than stating the facts to which I desire to draw attention, hoping thereby to induce medical men to watch cases of paralysis that may occur to them, with a view of still further elucidating this not uninteresting question.

"One of the patients, a man above forty, labouring under cerebral disease—probably incipient softening of the hemispheres—not accompanied by acute or violent symptoms, was, at the time of the inquiry, affected with sensory paralysis and partial motor paralysis, the latter more marked in the right than the left lower extremity. The loss of sensibility appeared about equal in all four extremities; but on applying the æsthesiometer, I found that the man was perfectly and readily able to distinguish a little more than one tenth of an inch at the tips of his fingers, a distance but little greater than that which healthy persons, whose touch has been more cultivated, can distinguish.

"The other patient is a young woman, aged twenty-one, who for about a year has been labouring under progressive paralysis, attacking first the motor powers of the lower extremities, and gradually extending upwards to the arms. Galvanism, applied in feeble currents to the different muscles, produces a ready reaction; the palsy cannot, therefore, depend upon a primary degeneration of the muscles, a view that is also negatived by other circumstances. Moreover, reflex action is readily excited in the legs. The question of diagnosis here is one of intense pathological interest, and would in itself deserve to be treated in a special paper. The point of the present communication is contained in the statement that in this girl common sensibility is actually exalted; but that, although she feels the prick of the æsthesiometer very acutely, she is unable, if the two points are widely separated, and even made to impinge upon different fingers, or on a finger and a part of the hand, to recognise more than one impression at a time. She has once or twice

said that she felt the application of two points as a scratch ; but taken in conjunction with other tests to determine her power of touch, it is manifest that, if not absolutely destroyed, it is much impaired, for she is unable to determine whether a surface is rough or smooth. On the legs she cannot distinguish two points, even at a foot apart.* The perception of temperature remains.

"I do not see how we can explain these cases otherwise than by assuming that there are two kinds of cutaneous sensibility—one destined to warn us by pain, the other by the various impressions which we can sum up by no better term than tactile sensibility or touch,—for which there are two distinct kinds and sets of conductors to carry them to the cerebrum. In the one case, sensibility to pain is diminished or destroyed, and touch persists ; in the other, the sense of touch has been annihilated, and the sense of pain remains."

ART. 45.—*On nervous disorders and nervousness lapsing into Melancholia and Insanity.* By Dr. J. TATUM BANKS, Physician to the Metropolitan Dispensary.

(London, Churchill, 12mo, pp. 55, 1858.)

In this essay Dr. Banks insists upon the imperative necessity of treating each particular case upon its own individual merits, and if this be done, he says "it will be found that nervous affections, and, in its early stage, insanity itself, are quite as amenable to medicine and as curable as any other severe disease." Dr. Banks has treated his subject in a popular and pleasing manner, and enforced his views by the narration of several instructive cases.

(B.) CONCERNING THE RESPIRATORY SYSTEM.

ART. 46.—*A new symptom of Croup serving as an indication for Tracheotomy.* By M. BOUCHUT.

(Gaz. Méd. de Paris, Aug. 7, 1858.)

The third period of croup, says M. Bouchut, is accompanied with general anæsthesia of the skin. This anæsthesia augments in proportion as the fibrinous secretions in the trachea accumulate, and it is produced by the impediment which is thus caused to the free entrance of blood into the lungs. The anæsthesia, indeed, is the sign that the defect of respiration has already proceeded to a serious degree, and this symptom therefore is regarded by M. Bouchut as a reason for having recourse to tracheotomy.

ART. 47.—*On the introduction of a Tube into the Larynx in Croup.* By M. BOUCHUT.

(L'Union Méd., No. 110, 1858 ; and Medical Times and Gazette, Oct. 30, 1858.)

M. Bouchut has laid before the Académie de Médecine a proposition to treat croup by introducing a silver tube deep into the larynx. Having

* In both cases the necessary precautions were taken to prevent any collusion, which in the girl was the less possible because she is all but blind.

ascertained the facility with which this may be done upon the dead subject, he put the practice into force in two cases of membranous croup brought into the Sainte Eugénie Hospital during the virulent epidemic that has recently prevailed in Paris. He employs three descriptions of instruments: a small curved catheter, open at both ends, as a director for the tube; a straight cylindrical tube, one and a half or two centimeters long, furnished at its upper part with two projecting rims (one placed around its orifice and the other at six millimeters below) and pierced with a hole for the passage of a retaining silk; and a protecting sheath for the index finger or dental dilator. Having ascertained on passing the tube in the subject that it entirely entered the larynx, its upper margin being placed below the superior corda vocalis, the inferior corda fitting into the space between the two rims of the canula, and consequently above the lower rim corresponding to the internal surface of the cricoid cartilage. M. Bouchut employed it in the case of a little girl brought in with diphtheria and croup in the stage of asphyxia. The canula remained *in situ* during thirty-six hours without inducing suffocation or interfering with the functions of the epiglottis, and the symptoms of asphyxia disappeared. The larynx was thus freed from the obstructing false membranes, and the croup must be considered as having been cured. The child, however, died from the diphtheria and from a lobular pneumonia set up the night after the tube was introduced. The second case occurred in a boy $3\frac{1}{2}$ years old, in whom symptoms of asphyxia had commenced. Improvement soon took place, false membranes of a large diameter being discharged through the tube, which remained *in situ* during forty hours, with the exception of one temporary removal, and never becoming displaced during coughing. At the end of this period, the fits of suffocation and asphyxia, which had been kept off for two days by means of the tube, returned; and the Internes, believing death to be imminent from obstruction occurring below the glottis, performed tracheotomy. Some false membranes were thus removed, and the tube was found to have remained in its place unobstructed.

M. Bouchut considers that these two facts establish—1. The facility with which tubage of the glottis may be performed by fixing a canula on the lower cordæ vocales, which does not interfere with the functions of the epiglottis. 2. The tolerance of this tube by the larynx. 3. The possibility of relieving the asphyxia of croup by this means in preference to tracheotomy. 4. The facility with which large pseudo-membranous concretions formed in the trachea and bronchi may pass through this intra-glottal tube. 5. The utility of this new resource for surgeons residing in remote localities, destitute of all assistance.

ART. 48.—*On the employment of Chlorate of Soda as a local application to the trachea after Tracheotomy in Croup.* By M. E. BARTHEZ.

(Gaz. Méd. de Paris, Aug. 14, 1858.)

M. Barthez employs a solution of from three to four grammes of chlorate of soda in thirty grammes of distilled water. He injects a few drops of this fluid through the canula into the trachea, and he does this to promote the solution of the fibrinous exudation. He thinks, indeed,

that the chlorate of soda has a very marked power in favouring this solution, but the evidence adduced on this subject cannot be regarded as conclusive.

ART. 49.—*On the fluctuations in attacks of Pneumonia between 1836 and 1856.* By Dr. ZIEMSEN, of Berlin.

(*Edin. Med. Journal*, Oct., 1858.)

It is no novel observation that pneumonia, at times a comparatively rare disease, is at others a strikingly frequent one, without, however, ever accumulating to any considerable epidemic. The details of this fluctuation have not yet, however, been definitively determined for any one special place, and far less have any comparative observations been made on the simultaneous oscillations in several places. In fact, the only material hitherto existing as applicable for such a purpose (the hospital lists), has been alike insufficient in point of numbers, and unfit from its composition; inasmuch as, to obtain a comparable quotient, an inconstant number of total sick must be employed, from which it follows that, by the prevalence of any other local epidemic, or by mere chance in the admissions, the true relative frequency of pneumonia may be easily obscured; while, on the other hand, the hospital sick being obtained from variable fractions of the respective populations, do not represent with certainty its occurrence in the entire population. The course of any disease, in any given place, can only be made obvious by comparing the total number of cases with the total of the population existing at that time; this is only attainable by making use of the national mortality tables which have of late years been issued by various governments. Dr. Ziemssen, in making use of these, has collected a total of 376,800 fatal cases of pneumonia, occurring in 15 European towns, 2 European countries, 1 North American, and 1 North African town, during the period of 21 years, 1836 to 1856 inclusive, forming a total period of observation of 178 years; for the sake of comparison, a series of 7000 pneumonic attacks (not deaths) has also been extracted from the same period. These valuable tables (which we omit) throw a certain amount of light on the following five questions:

- I. The amount of fluctuation in mortality from pneumonia.
- II. The relation of the amount of fluctuation in the mortality from pneumonia to that of other diseases.
- III. The agreement and difference in the annual mortality from pneumonia during the 21 years, 1836 to 1856, in the different places.
- IV. The type of oscillation in the mortality from pneumonia.
- V. The parallelism of the curve of fluctuation of pneumonia with that of the curves of other diseases in the same period.

I. The mortality from pneumonia undergoes constant and considerable fluctuations, which vary, nevertheless, in each different place. To reduce these to a comparable mean, two computable conditions require to be considered.

(a) The amount of the *extreme fluctuation*, *i. e.*, the numerical difference between the highest and the lowest relative yearly loss of a population from pneumonia. To obtain this, the minimum mortality (Tab. I) is

subtracted from the maximum, and the resulting cypher of extreme fluctuation is reduced to a per-centage relative to the mean annual material; this per-centage is then the comparable measure of the extreme fluctuation. Rejecting places where the observations have been carried on too short a time, and such as have too trifling an amount of population (Leith, Belfast, Limerick, Breslau), the others rise in the amount of their extreme fluctuation in the following order:

TABLE I.

Duration of the observation in years.	Place of observation.	Mean annual material.	Maximum.	Minimum.	Extreme fluctuation.	Measure of the extreme fluctuation.
11	All England .	1.25	1.44	1.11	0.33	26.4%
5	Algiers . . .	2.33	2.61	1.62	0.99	42.5 „
9	Hamburgh . .	2.17	2.56	1.60	0.96	44.2 „
9	Zürich . . .	2.05	2.66	1.69	0.97	47.3 „
14	Paris	2.56	3.26	2.05	1.21	47.3 „
10	Boston	1.54	2.05	1.24	0.81	52.6 „
21	Berlin	1.14	1.46	0.86	0.60	52.7 „
4	Gent	1.21	1.68	1.02	0.66	54.5 „
3	Edinburgh . .	1.42	1.75	0.89	0.86	60.6 „
17	London	1.69	2.12	0.96	1.16	68.6 „
10	Halle	2.29	3.41	1.47	1.94	84.7 „
9	Dublin	0.81	1.28	0.52	0.76	93.8 „
9	All Ireland . .	0.27	0.41	0.14	0.27	100.0 „
15	Copenhagen . .	1.73	2.56	0.71	1.85	106.9 „
9	Cork	0.46	0.78	0.28	0.50	108.7 „
155	Mean of totals .	1.53	2.00	1.08	0.92	60.1%

In the towns and countries of the Northern Hemisphere, the variation in the annual mortality from pneumonia may thus exceed, on the average, the mean annual mortality from that cause by 3.5ths.

(b) Of still more importance is the determination of the *mean fluctuation*, i. e., the mean difference between two following annual periods. It is obtained by adding together the successive differences (both increase and decrease) between each annual period of the given place of observation, dividing this sum by the number of these differences, and reducing the result to a per-centage relative to the mean annual material. Excluding places where the duration of the observations has been less than nine years (Algiers, Gent, Edinburgh, Leith, Breslau), the others follow in the following order:

TABLE II.

	Mean fluctuation.	Measure of the mean fluctuation.	For comparison. (measure of the extreme fluctuation).
All England	0·08	7·0%	26·4
London	0·22	12·8 „	68·6
Zürich	0·26	13·0 „	47·3
All Ireland	0·04	14·8 „	100·0
Paris	0·38	15·2 „	47·3
Copenhagen	0·27	15·8 „	106·9
Hamburgh	0·42	19·5 „	44·2
Dublin	0·17	20·6 „	93·8
Berlin	0·23	20·6 „	52·7
Cork	0·10	21·8 „	108·7
Boston	0·37	24·2 „	52·6
Limerick	0·17	28·3 „	163·3
Halle	0·69	30·3 „	84·7
Mean of totals . . .	0·26	18·8%	

If we suppose, then, that the mean annual mortality from pneumonia in the temperate zone of the Northern Hemisphere amounts to = 100, each year may differ on the average by 19 from its predecessor or successor, *i.e.*, the average variation from one year to another amounts to nearly 1·5th of the mean annual amount; 19 per cent., therefore, of the annual mortality from pneumonia in Europe is dependent on change of season.

Different places vary very much in this respect, the causes of pneumonia appearing to act twice as uniformly in England as in Ireland, while in Halle the dependence of its mortality on season seems to be twice as great as in Paris, &c. The source of the increase in the mean fluctuation cannot with certainty be determined, and all the less so that the periods of observation are dissimilar, and do not always comprise the same seasons; still, the following provisional conclusions may be drawn:

1. The amount of the mean fluctuation in the mortality from pneumonia is in inverse relation to the circumference (density?) of the population. Thus, the three countries, England, Ireland, and Canton Zürich, have a mean fluctuation of 11·6 per cent., the ten towns one of 20·9 per cent. The six larger European towns (London, Paris, Hamburgh, Copenhagen, Dublin) have a mean fluctuation of 17·4 per cent.; the four smaller towns (Halle, Cork, Limerick, Boston), one of 26·1 per cent. The countries have also always much less fluctuation than their contained towns—Ireland 14·8 to Dublin 20·6; England with 7·0 to London with 12·8. General climatic relations, particularly the differ-

ence between oceanic and continental climates, seem to have no influence on the amount of the mean fluctuation, as it is relatively the same for Zürich and London, Berlin and Dublin. The amount of the extreme fluctuation has no direct relation to the amount of the mean fluctuation. Places like Copenhagen may have little variation from year to year, and yet have extreme maxima and minima; while others, like Berlin, may vary much from year to year, and yet have no remarkable extreme. The difference between the maximum and minimum mortality cannot, therefore, ever be an efficient measure of the amount of uniformity with which the causes of pneumonia act in any given place. There is nothing to give determinate support to any conjectures respecting the cause of these extreme fluctuations: Halle is relatively the same as Dublin; while Hamburgh and Copenhagen, and England and Ireland, differ widely, though lying near one another and having similar climatic conditions.

II. *The relation of the amount of fluctuation in mortality from pneumonia to that of other diseases* which are frequent sources of mortality, Dr. Ziemssen has deduced from the mortality tables of London, obtaining the measure of the fluctuation in the manner already mentioned, after reducing the figures directly obtained to those having reference to 100,000 living—the period of observation comprises fifteen years, 1840-54:

TABLE III.

	Mean an. mortality in 100,000 living.	Mean fluctua- tion.	Extreme fluctua- tion.	Measure of mean fluctua- tion.	Measure of extreme fluctua- tion.
Phthisis	3.23	0.14	1.39	4.5%	40.1%
Apoplexy	0.52	0.04	0.18	6.7 „	34.5 „
Pneumonia	1.70	0.23	1.16	13.7 „	68.2 „
Bronchitis	1.15	0.25	1.81	22.0 „	158.0 „
Typhus	0.95	0.22	0.98	22.9 „	103.6 „
Diarrhœa	0.75	0.22	1.28	29.4 „	170.8 „
Hooping-cough . . .	9.86	0.27	0.64	31.0 „	74.1 „
Measles	0.57	0.24	0.87	41.7 „	151.8 „

From these figures, we see that though pneumonia is far from reaching the amount of fluctuation of such epidemic diseases as measles or hooping-cough, yet its fluctuations are very much greater than those of such diseases as phthisis and apoplexy, which arise from constitutional and general circumstances affecting vitality; pneumonia stands next to typhus, and is but little behind it in irregularity of occurrence.

III. By comparing the relative mortality from pneumonia in the different places, Dr. Ziemssen has deduced the fact that pneumonia, in spite of many irregularities and apparent exceptions, passes through simultaneous waves of increase and decrease, in northern as well as in southern countries, in insular as well as in continental climates, in North America,

Europe, North Africa, and the East Indies, and this so unmistakeably, that we are led to consider these waves as dependent on general causes which move simultaneously over the entire northern hemisphere. This Dr. Ziemssen fully illustrates, by entering minutely into the course of pneumonia in each of the twenty-one years, 1836-1856.

Further consideration of the *share taken in these oscillations by the different sexes and ages*, brings to light—(a) That the mortality among the female sex varies with the season, and that in so strikingly regular a manner, that, when transmuted into curves, the curve of mortality among females, from pneumonia, is almost perfectly concentric with the curve of general mortality from the same cause, showing that those influences which favour the origin of pneumonia act with peculiar intensity on the female sex.

(b) Inasmuch as but few mortality tables class the fatal cases of pneumonia according to their ages, and the comparative use of these few facts is rendered more difficult by different classifications of the ages being made, not only in different places, but in the same place in different seasons, so only an approximation on this head can be arrived at. From this it seems (1.) that the mortality from pneumonia in childhood is more liable to fluctuation than that of adults, in the proportion of 28 : 20 or 7 : 5. (2.) The fluctuation in the mortality of both classes (children and adults) is not always parallel, *i. e.*, pneumonia may rage in the one and not in the other; and this sometimes affords an explanation of a seeming irregularity in the course of pneumonia; for instance, in 1841, when the mortality from pneumonia was generally at its ebb, the curve of Hamburgh, as a sole exception, culminates. This was the effect of an entirely local epidemic of pneumonia amongst the children, of whom 22 per cent. above the annual average died, while, in unison with every other place, the mortality amongst the adults from the same cause was 17 per cent. below the average. (3.) Fluctuations in the pneumonia of adults in the various places are tolerably parallel; on the contrary, infantile (0-10) pneumonia varies widely even in places socially and climatically related, such as Hamburgh and Copenhagen; and this epidemic occurrence of pneumonia within territorial limits, recalls similar movements in acknowledged contagious diseases, as measles and scarlatina, and can scarcely be explained by any of the known modifications of atmospheric influence.

IV. In regard to the *type of the pneumonia movement*, Dr. Ziemssen considers himself justified in regarding it as irregular and not periodic; at the same time it is remarkably characteristic of pneumonia, one culmination point occurring in most places every third year. This peculiar oscillation in pneumonia gives the impression, that possibly each place may possess a determinate mean mortality from this cause, the ever varying annual mortality always, in shorter or longer recurring periods, bringing out this same endemic mean; for instance, in Berlin, there died of pneumonia, in twenty-one years, to 100,000 living—

In the quinquennium	1836-40 . 1.13	} In the 1st decennium . 1.14
„ „	1841-45 . 1.15	
„ „	1846-50 . 1.13	} In the 2d decennium . 1.14
„ sexennium	1851-56 . 1.16	

But this impression is, of course, liable to be overset by many unforeseen possibilities.

V. A comparison of the mortality of other diseases with that of pneumonia leads to the conclusion that the curve of pneumonia is by no means concentric with that of other inflammatory diseases; on the contrary, that exquisite pneumonic years are characterised by a rarity of other inflammations, which, moreover, fluctuate but slightly. On the other hand, many pneumonic years are characterised by an unusual frequency in other diseases of the respiratory organs; and also many years in which pneumonia culminates in other places, but not in London,* are also those distinguished by increased affections of the respiratory organs, though at other times, as in 1843, this is not the case; in 1843, however, typhoid diseases were remarkably frequent. The curves of typhus and pneumonia are, moreover, remarkably parallel; and not only so, but years marked on the Continent by increase of pneumonia, and not so in London, are those marked by an increase of typhus. This parallelism is the more remarkable, as hitherto these two diseases have been regarded as representatives of opposite and, to some extent, antagonistic morbid constitutions. From these facts, we gather that pneumonia will then be most frequent when influences predisposing to diseases of the lungs combine with others which dispose to typhus, of which a remarkable example is afforded by the prevalence of an almost double the previous mortality from pneumonia in Denmark and Ireland during the famine years 1845-59, when typhus, dysentery, intermittents, and scurvy raged.

In further investigating the oscillations in the mortality of pneumonia, month by month, Dr. Ziemssen comes to the conclusion, that once or oftener in each year, a period of unusual excess alternates with one of deficit; and in the culminating years themselves, it is extremely rare that every or almost every month is above the average, and much more usually single months influence by their decided excess or deficit the entire amount of the annual mortality. These minor epidemics are wonderfully simultaneous in the various places, seem to be almost as frequent in summer and autumn as in winter and spring, but attain in the two latter quarters about thrice the extent they do in the two former ones.

ART. 50.—*On the bleeding and non-bleeding plans of treatment in Pneumonia.* By Dr. ———

(*Medico-Chir. Review*, July, 1858.)

The following numerical particulars are appended to a valuable review on the bloodletting controversy.

The non-bleeding plan, the reviewer considers, has a demonstrable advantage over that of indiscriminate and repeated bleeding; but at the same time he maintains that the discriminate practice of moderate early bleeding, general or local, is pressed upon us both by expe-

* In this comparison the mortality tables of London are alone made use of. Under the head "Inflammations" are comprised cephalitis, pericarditis, laryngitis, pleuritis, hepatitis, peritonitis, gastritis, and enteritis; under that of diseases of the respiratory organs, pneumonia, tuberculosis, hooping-cough, and croup.

rience and science, in cases of more or less sthenic pneumonia. He maintains also that bloodletting is admissible in asthenic pneumonia, whether as regards the character of the disease or the constitution of the patient.

Bleeding Plan.

	No. of cases.	No. of deaths.	One death in
<i>Few bleedings.</i>			
Bouillaud (Pelletan)	71	9	7.9
Ditto (own report)	102	12	8.5
Lacaze	42	1	42
Briquet	129	17	7
Chomel (Louis)	78	28	2.8
Ditto (Grisolle)	50	14	3.6
Ditto (ditto) [T.E.]	24	13	1.8
Louis (20 cases T.E.)	29	4	7.2
Grisolle	232	37	6.3
Piorry	30	2	15
Rasori [T.E.]	747	164	4.5
Ditto Wagner [T.E.]	13	7	1.9
Acerbi [T.E.]	142	16	8.9
Skoda [T.E.] females	64	1	64
Do. [do] males & fem.	—	—	8
<i>Repeated or large bleedings.</i>			
Laennec [T.E.]	62	6	10.3
Do. [T.E.] Leconteulx	32	12	2.7
Grisolle [T.E.]	110	28	4
Ruef [T.E.]	94	5	18.8
Bang [T.E.]	54	2	27
Gerhard (children 6 to 16)	26	1	26
Becquerel (children)	12	8	1.5
Ditto, ditto (secondary)	16	15 or 16	
Dietl	85	17	5
Wunderlich	47	3	15.6
Ditto*	114	9	12.7
Dr. Bell [M.]	36	0	0
Wossidlo	112	4	28
Burkart	60	1	60
De Bordes†	13	2	6.5
Müllert	10	0	0
Forget	32	7	4.6
Schmidt‡	6	1	6
Morehead, Dr. (local)	57	11	5.2
Ditto, total (bled or not)	103	32	3.2

Non-Bleeding Plan.

	No. of cases.	No. of deaths.	One death in
Trousseau [T.E.]	52	2	26
Grisolle [T.E.]	44	6	7.3
Thielmann [T.E.]	110	12	9.2
Schmidt [T.E.]	87	4	9.1
Ditto [O.]	13	6	2.1
Ditto [T.E., O.]	46	9	5.1
Dietl [T.E.]	106	22	4.8
Skoda (Balfour), 1146	45	3	15
Ditto (Mitchell), 1852	54	14	3.9
Oppolzer (ditto)	32	5	6.4
Vienna Hospital (ditto)	240	40	6
Sigmund, Pr. & Ho. (ditto)	743	104	7
Vienna Hospital (ditto)§	5909	1439	4.1
Ditto, 1856	658	187	3.5
Wittich [M.]	23	0	0
Bell, Dr. [M.]	35	4	8.7
Gandini [M., O.]	25	0	0
Cohn [T.E., O.]	57	24	2.4
De Bordes [O.]	77	17	4.5
Burkart [O., L.]	60	1	60
Baumgärtner [Ch.]	30	3	10
Wucherer [Ch.]	90	1	90
Helbing [Ch.]	62	5	12
Varrentrapp [Ch.]§	23	1	23
Stoham [Ch.]	30	0	0
Frohmüller [Ch.]	12	0	0
Hutava [Ch.]	12	0	0
Theile [Ch.]	6	0	0
Salawa [Ch.]§	15	0	0
Dietl [diet only], 1848	189	14	13.5
Ditto [ditto], 1852	750	69	10.9
Wieden Hos. Vienna, 1855	—	—	4.8
Bennett, Dr.	65	3	21.7
Wunderlich	76	13	5.8
Kissel [L., Co.]	112	5	22

ART. 51.—*On the influence of Solidification of the Lung upon vocal vibration.* By Dr. GEORGE JOHNSON, Physician to King's College-Hospital, &c.

(Dr. Beale's *Archiv. of Med.*, No. II, 1858.)

"Until within the last few months," says Dr. Johnson, "I have been in the habit of considering that, as a rule, the vocal vibration or fremitus over solid lung is greater than over the corresponding part of the healthy

* Combined general and local bleeding, and spontaneous hæmorrhage.

† Part bled generally, part locally.

‡ Four bled before admission.

§ Very few bled.

T. E. Tartar-emetic.

Ch. Chloroform.

M. Mercury.

I. Iron.

Co. Copper.

O. Opium.

L. Lead.

lung, and that the exceptions to this rule are rare; further, that in this sign we have a valuable means of distinguishing between solid lung and liquid in the pleura, the vocal vibration in the latter condition being invariably diminished.

"I believe still, that in most cases there is increased vocal vibration over solid lung, but I think that the exceptions to this rule are more numerous than is commonly supposed; and I have been led to this opinion by finding, that out of twelve cases of pneumonia which have occurred in my hospital practice during the last eighteen months, in three there has been decided *diminution* of the vocal vibration over the hepatized lung; while in two others the vibration was neither increased nor diminished, as compared with the corresponding part of the opposite side. The following is a brief outline of the most remarkable of these cases:

"CASE 1.—Edward C—, æt. 17, was admitted on the 12th December, having been four days ill. He had all the general symptoms of pneumonia. On the day of admission, the physical signs were these: dulness on percussion over the whole lower lobe of the left lung, from the spine of the scapula downwards; over the same space distinct bronchial respiration and voice, vocal vibration *less* than on the sound side. Over the front of the left side—*i. e.*, the upper lobe of the left lung—percussion natural, respiration vesicular, clear and loud, vocal vibration *much stronger* than over the corresponding part of the right side.

"On the 17th, the physical signs over the lower lobe were the same as before, but the upper lobe had become solid as shown by the following signs: great dulness on percussion over the whole left front; bronchial respiration and voice, and now *diminished* vocal vibration. The heart continued to beat in its natural position. The movements of the left side of the chest were much less than those of the right. The physical signs remained unchanged until the time of the patient's death, which occurred on the 24th December.

"On post-mortem examination, the whole left lung was found solid, and of a mottled grayish colour. The pleura was covered by recent lymph, which formed a very thin layer, except in some of the intercostal spaces, where it was about one eighth of an inch thick; there was no liquid in the cavity of the pleura.

"What was the cause of the diminished vocal vibration over this solid lung? I have no explanation to offer, but I would remark, that it can scarcely be supposed that obstruction of the bronchial tubes was the cause of the phenomenon, inasmuch as bronchial respiration was heard over the whole left side, with a distinctness rarely observed when the entire lung is solidified by inflammation.

"I wish to direct attention particularly to one phenomenon in this case—namely, that during the first few days, while the lower lobe alone was solid and the vocal vibration over it was diminished, there was a great increase of vocal vibration over the *upper* lobe of the same side, which as yet was free from disease; but when this upper lobe became solid, the vocal vibration over its surface was as much diminished as it had before been and continued to be over the lower lobe. In two others of the twelve cases of pneumonia to which I have referred, it was also observed that hepatization of the lower lobe of one lung had the effect of greatly increasing the vocal vibration over the upper lobe of the same

side, the sounds of percussion and respiration over the upper lobe remaining quite normal. In one of these cases, the vibration over the solidified lower lobe was much diminished; in the other, it was neither increased nor diminished. In both cases, the disease was on the right side; but the increase of vocal vibration over the right upper lobe appeared to be greater than the natural excess of vibration on that side.

"I have lately observed similar phenomena in a case of acute tubercular disease of the lung, which is further remarkable on account of the tubercular deposit being more abundant in the lower than in the upper lobe of the lung. The following is a summary of the case:

"CASE 2.—Charles Wood, æt. 42, a carpenter, had been ill six weeks before his admission, suffering from cough, with scanty expectoration, fever, loss of flesh and strength; he had continued his work until three weeks ago; admitted November 12th. He was much emaciated, and had a sallow, unhealthy appearance, skin hot, tongue red and dry, P. 116, R. 32. Decided dulness on percussion over the lower lobe of the left lung; slight dulness over the left upper lobe, as compared with the opposite side. Bronchial breathing and voice near the angle of the scapula on the left side; *diminished* vocal vibration over the same space; over the left upper lobe in front, large crepitation, with *increased* vocal vibration.

"On the 17th, the respiration at the angle of the scapula was doubtfully cavernous, and gurgling was heard in the same situation. He had expectorated only a few pellets of gray mucus.

"The physical signs remained the same until his death, which occurred on the 23d.

"The lower lobe of the left lung was solidified by a tubercular deposit, which at the base had completely infiltrated the pulmonary tissue. At the upper margin of this lobe was a cavity the size of a walnut; some smaller cavities near this; a considerable tubercular deposit in the upper and back part of the left upper lobe; this was breaking down into small cavities, the largest the size of a marble. The lower and anterior part of this lobe was gorged, but crepitant. There was a scanty deposit of crude tubercle in the apex of the right lung.

"With respect to the increased vocal vibration over the upper lobe of the left lung, it may perhaps be objected that this was the result of the tubercular disease and engorgement of this lobe. Still, the fact remains, that over the lower lobe, which was much more solid than the upper, the vibration was decidedly less than over the corresponding part of the opposite side.

"Another case in which we have lately found complete absence of vocal vibration over solid lung was one of primary cancer of the lung.

"CASE 3.—John Robinson, æt. 41, a painter, admitted June 7th, 1856. Five weeks before admission, after exposure to cold and bad living, he began to suffer from pain in the limbs and in the right side of the chest. The pain in the chest continued, and he rapidly lost flesh.

"On admission, he had a pale, unhealthy aspect; his chief complaint was of pain on the right side of the chest, and there was a hard swelling about the size of a walnut over the eighth and ninth ribs, just in front of their angles. This swelling was extremely tender. Dulness on percussion over the whole lower lobe of the right lung; at the upper part of this lobe the dulness was less complete than over the lower two thirds. Elsewhere there was the normal resonance. Over the lower two thirds of the inferior right lobe there

was complete absence of respiratory and voice sounds, and of vocal vibration. At the upper part of this lobe—that is, just below the spine of the scapula—there was indistinct respiration, with some crepitation, and the voice had a bronchophonic character.

“The patient continued to lose flesh and strength; he coughed and expectorated puriform mucus, tinged with florid blood, which often gave it the colour of red-currant jelly. The skin was always cool; the pulse usually 104, and the respiration 24.

“The physical signs over the lower lobe of the right lung remained essentially the same; but the dulness towards the upper part of the lobe rather increased, and the right side became decidedly flattened. The hard tumour before mentioned grew to be as large as an egg.

“On the 24th July, the patient being then much emaciated, it was first noted that, when he was in the recumbent position, there was a distinct wave-like pulsation in the right jugular vein, and in one of the thyroid veins on the right side. This venous pulse ceased when he sat up.

“On the 29th July, it was further observed that there was dulness, on percussion, and tubular breathing below the sternal end of the right clavicle.

“The expectoration had become more copious, and had a dirty, purulent character. It was frequently examined with the microscope, and was found to contain pus, granular fat-cells, and epithelium, but no fibrous lung-tissue, nor any products which appeared to be of a cancerous nature. Yet the history of the case, the physical signs, and the hard tumour on the right side, left little room for doubt as to the disease being cancer; and that opinion was expressed in a clinical lecture while the patient was living.

“He gradually sank, and died on the 4th August.

“The whole lower lobe of the right lung, except at its upper margin, was entirely occupied by a cancerous growth, which was universally adherent to the wall of the chest, projecting between the seventh, eighth, ninth, and tenth right ribs, to form the tumour before mentioned, and forming another projection as large as an orange, which had made a depression in the upper surface of the liver. The centre of the cancerous mass was softened, and contained about twenty ounces of creamy fluid. This had no means of escape until the tumour was broken through in tearing it from its adhesions to the walls of the chest.

“A mass of enlarged lymphatic glands formed a tumour, which lay behind the right sterno-clavicular joint, touched the trachea, and passed down between the right brachio-cephalic vein and the ascending aorta, its position between these two vessels being such that it must evidently have communicated a pulsation from the artery to the vein. This state of parts had been anticipated from the phenomena observed on the 24th and 29th August.

“The only remaining point to be noticed is, that in the apex of each lung there were some indurated remains of an old tubercular deposit. In this case, the complete absence of respiratory and vocal sounds, as well as of vocal vibration, is accounted for by the entire obliteration of the pulmonary tissue through nearly the whole lower lobe of the right lung.

“The cases to which I have referred will suffice to show that the vocal vibration over solid lung is by no means uncommonly diminished, and it is evident that the diagnosis between solid lung and liquid in the pleura is often but little aided by a comparison of the vocal vibration on the two sides of the chest.

“It has occurred to me on more than one occasion to find that pneumonia of the lower lobe of the lung has been mistaken for pleurisy with

liquid effusion—a mistake which is the more likely to occur when there is diminished vocal fremitus over the solidified lung. There is one help towards a correct diagnosis in cases of this kind, which has, I think, been too little regarded by those who have written on the subject. In percussing the chest, we should bear in mind the form and limits of the lobes of the lung, and ascertain whether the extent of dulness corresponds with the outline of one or other of these lobes. If this be found to be the case, the probability is, that the dulness depends on solidified lung, and not on liquid in the pleura. Take, for instance, a case in which the lower lobe of the left lung is hepatized. It will be found, that while percussion over the back of the chest elicits a dull sound from the spine of the scapula downwards, in front there is the natural resonance, and the lateral region is partly dull and partly resonant; the boundary line between the dull and the resonant part extending obliquely downwards and forwards from just below the spine of the scapula, in the direction of the fissure between the two lobes of the lung. It can rarely, if ever, happen that liquid in the pleura is so circumscribed as that, while it extends as high as the spine of the scapula at the back, it will leave the front of the chest normally resonant when the patient is in the erect posture. When the upper lobe alone of the left lung is solidified, the line of demarcation is the same as in the other case, but the dull and the resonant parts are reversed. The middle lobe of the right lung, extending in front from the mamma downwards, may be inflamed and solidified alone, as happened in one of the twelve cases before mentioned, or it may be affected at the same time with either the upper or the lower lobe.

“My object in the present communication has been to direct attention to three practical points relating to the diagnosis of pulmonary disease. 1st. That the vocal vibration is not unfrequently diminished over solid lung. 2d. That when the *lower* lobe of the lung is solidified, the vocal vibration over the *upper* lobe of the same side is sometimes remarkably increased. 3d. That in percussing the chest, when the question of diagnosis is between solid lung and liquid in the pleura, it is important to remember the exact form and position of the several lobes of the lung.”

ART. 52.—*Diagnostical and prognostical value of Hæmoptysis.*

By Dr. TROUSSEAU, Physician to the Hôtel Dieu, Paris.

(*L'Union Médicale*, Nos. 110 and 113, 1857.)

Professor Trousseau states that, on finding an individual spitting blood, the first idea that presents itself is the existence of pulmonary tubercle; but if we note all the cases that present themselves, not in private, but in hospital practice, we shall find the hæmoptysis as often dependent upon other causes as upon tubercular disease. This statement, paradoxical as it may seem, is quite true when confined to hospital patients.

A form of hæmoptysis that is rarely met with in hospitals, is due to *hæmorrhagic deviations*. We meet with women who, without suffering from any notable disturbance of menstruation, but who are the subjects

of nervous symptoms, spit blood frequently in considerable quantity. Neither the symptoms nor attentive exploration of the chest, indicate any affection of the heart or lungs; and when the period of menopause arrives, the hæmoptysis becomes arrested, and does not return. Other women spit blood during pregnancy or lactation, and cease doing so when these conditions are terminated. These nervous women are also sometimes the subject of menorrhagia, seeming to be under the influence of a hemorrhagic diathesis; and when the critical discharge does not take place by the uterine mucous membrane, it does so by the bronchial membrane. Although these hæmoptyses are not of the importance that might be supposed, and may be reproduced at longer or shorter intervals during several years, it must be borne in mind that this frequent repetition may induce a congested state of the respiratory apparatus, during which even a slight accessory cause may give rise to a more or less dangerous phlegmasia. Independently of abnormal circumstances, we may meet with hæmoptysis occurring, so to speak, as a physiological accident, supplying the place of a natural or accidental discharge of blood, which, from some cause or other, does not take place by the ordinary channel. Thus, in women with obstructed menstruation, it is one of the most frequent forms of *hæmorrhage supplementary to menstruation*. It will be readily understood that when with this peculiar disposition of the economy there is combined another dependent upon a local predisposing condition of the pulmonary apparatus, these hæmoptyses are still more readily produced. Under such circumstances the prognosis of hæmoptysis is far more serious than when it arises from hæmorrhagic deviations unconnected with local occasional causes. Here, in fact, the accidents become complicated with the local lesion which has led to these manifestations, just as this itself is necessarily complicated by the fact of the fluxionary hæmorrhagic movement, which, at each return, accelerates the evolution of such lesion.

As already observed, these varieties of pulmonary hemorrhage are rare in hospital practice. The form of the affection, however, there most commonly met with, is not hæmoptysis dependent upon phthisis, but hæmoptysis dependent upon disease of the heart. It is not meant by this to declare in an absolute manner, that tubercular hæmoptysis is of rarer occurrence than hæmoptysis dependent upon disease of the heart, but only to state that in phthisis, hæmoptysis being in general a transitory condition, occurring early in the affection, the patients do not come to the hospital, while hæmoptysis dependent upon heart-disease occurs principally when the disease is much advanced, and, consequently, at the period when patients are obliged to resort to the hospitals. Proceeding to consider some of the points of diagnosis between these two forms, we find that in youth, adolescence, and the early period of mature life, from the sixteenth to the fortieth year, hæmoptysis most generally is dependent upon pulmonary tubercle, and that whether it is met with in hospital or private practice; but after the fortieth year, and still more after the fiftieth, it is no longer, generally at least, a sign of phthisis, but of disease of the heart. There are exceptions to this rule, but they do not invalidate its general truth. In phthisis bloody expectoration may either precede any other manifestation of the disease, of which it may then be considered the earliest symptom, or it may appear in the

course of the affection. Laennec indicated its slight quantity as a characteristic, and regarded very abundant hæmoptysis as almost always due to pulmonary apoplexy. But he had little opportunity of observation in private practice. It is true that, in general, hæmoptysis is not abundant, but still there are cases in which it is overwhelmingly so, causing death by the sole fact of the loss of an enormous quantity of blood. Hæmoptysis, consequent on disease of the heart, is, notwithstanding, still seldomer overwhelming (*foudroyante*) than bronchial hæmorrhage. It may recur fifteen, twenty, forty, or fifty days in succession without at once proving fatal. Of course, when dependent upon the rupture of an aneurismatic vessel into the bronchi, it may prove still more rapidly fatal than hæmoptysis supervening on phthisis. Besides the age of the patient and the progress of the symptoms as elements in the differential diagnosis, there is an important point in regard to the seat of the hæmorrhage, viz., that while in phthisis it takes place generally at the bronchial surface, in disease of the heart it is most often parenchymatous, first occurring in the pulmonary vesicles.

As to the question of the characteristics of bronchial and pulmonary sanguineous expectoration, it is said that bronchial hæmorrhage is observed under the form of spumous, semi-fluid sputa, resembling blood beat up with air, and having a bright redness, deemed characteristic. The quantity discharged is said to be sometimes very slight, and sometimes very abundant, not being mingled with the débris of alimentary substances or mucosities. But this is far from being always the case, as the sputa may be as viscous as those seen in the first stage of pneumonia, or in pulmonary apoplexy, an appearance, probably, due to the slight accompanying inflammatory action, or to the accumulation and detention of the blood in the lungs. So, too, we may find the discharges mixed with alimentary substances when the hæmoptysis is undoubtedly connected with phthisis. Stethoscopic signs are often at default, or indicate as much, or even more, the pulmonary lesion upon which the hæmoptysis depends. Generally at the autopsy of persons who have been the subjects of bronchial hæmorrhage, we only find, besides the lesions proper to phthisis, redness of the pulmonary mucous membrane, which, indeed, may be due to imbibition. If cavities exist, they may contain a certain amount of coagulated blood, and that usually when vascular ruptures take place within these; otherwise we only find a little blood accumulated in the bronchi.

With respect to pulmonary hæmorrhage, we may advert to the erroneous term, "pulmonary apoplexy," which has been bestowed upon it, giving, as it does, no idea of the nature of the affection. It occurs in general during the course of an affection of the heart; and at the autopsy kernels of engorgement are found of as deep a colour as the spleen, and as hard as those of pneumonia in its second stage. The tissue of the lung is friable, and presents the granular aspect of hepatized tissue, except that while in the latter the vessels and lobular inter-sections are visible, the hæmoptical engorgement presents a uniform blackish, or very deep brown colour. This lesion, which would be better termed sanguineous infiltration, bears no analogy to cerebral apoplexy, the term apoplexy always implying the idea of suddenness and active fluxion, a condition rather belonging to bronchial than pulmonary

hæmorrhage, which is ordinarily, to a certain extent, passive. There are, indeed, cases of true pulmonary apoplexy giving rise to sudden death, and characterised by the effusion of more or less blood amidst the lacerated lung. The term apoplexy would be much better applied to cases of active congestion of the lung, a not very rare disease, but which is rarely accompanied by hæmoptysis, properly so called. Gendrin proposes to substitute for the term pulmonary apoplexy, *pneumo-hæmorrhage*, indicating without ambiguity an extravasation of blood into the tissue of the lungs. As to the distinctive signs in these cases of pulmonary sanguineous infiltration, the expectoration is generally viscous, sometimes red, and sometimes black, and even deep black. But, as in bronchial hæmorrhage, the blood discharged is also sometimes black, so in the pulmonary it is sometimes spumous, and that especially when it is quickly and abundantly discharged.

While lesions of the heart are the usual causes of pulmonary hæmorrhage, contraction and insufficiency of the mitral valve is the most common of these lesions, and especially when, as is commonly the case, it is conjoined with ventricular hypertrophy. These hæmorrhages are sometimes very considerable, and may recur three, four, six, eight, or ten times in the course of the disease of the heart; at other times, though rarely, they are slight and transitory, and do not reappear. When the lesion is much advanced, the patients may spit blood for one or two months, and sometimes until their death. The disposition of these hæmorrhages is, in fact, to increase in frequency and in quantity as the disease of the heart—an effect of which they are—approaches its fatal termination.

ART. 53.—*The diagnosis of Pulmonary Consumption at its commencement.*

By Dr. SCOTT ALISON, Assistant-Physician to the Hospital for Consumption at Brompton.

(*Lancet*, June 19, 1858.)

In this paper Dr. Scott Alison endeavours to show the importance of diagnosis at an early period by reference to facts arranged under three heads: 1st, the vast mortality in advanced stages; 2d, the great destruction of the lining structure almost invariably found when the disease has long existed, in a great proportion of cases excluding all reasonable hope of remedy; and 3d, the material benefit afforded in a very large proportion of cases easily diagnosticated and treated. The number of patients under the care of the author at the Hospital for Consumption at Brompton, who here formed the grounds for calculation, is nearly 2000. The mortality in advanced cases has been very great, and very few have presented signs of permanent restoration to health; whilst the mortality in early cases has been comparatively trifling, even when long observed. About one half of these latter cases have been greatly improved, and have presented satisfactory evidences of the disease being arrested. Numbers have returned to their employment, or applied themselves to less laborious and exposed occupations. Muscle and fat have greatly increased, cough has been removed, and the respiration has been deprived of much of its shortness. About three fourths of these

patients presented grave symptoms and the usual physical signs; while the remainder presented either well-marked physical signs without material symptoms, or very marked symptoms with physical signs rather beneath the average weight of evidence ordinarily deemed proof of phthisis. The author regards the results as due to the early period at which disease was diagnosticated, and not to any particular method of medical treatment. Early diagnosis would be secured by a complete inquiry into the history of each case, by regarding the entire series of symptoms, and by a complete physical examination instituted at once, the chest being freely exposed back and front. The present state of medical knowledge was such and so widely diffused, that it was not likely we should be able to find any new symptoms of the disease, one which had been carefully observed for ages; but it was not unlikely that we should increase our knowledge of the physical signs. In the particulars of sound, form, and motion, additions would probably be made; but it was with respect to sound that most advance would be effected. Simple observation by the present means of auscultation would probably suffice to do much, but it was not unlikely that improvements in our instruments for auscultation would render assistance. Dr. Alison referred to certain sounds which he had frequently heard in phthisis at its commencement, and before dulness of percussion had manifested itself, or was materially pronounced. These sounds were an "arrowroot-powder" sound, very fine, and accompanying expiration; buzzing, humming, and kettle-boiling or kettle-singing sounds. He was as yet uncertain as to the mechanism of the kettle-singing sound, but was inclined to think the evidence pointed to slight pressure on the veins of the lung, causing oscillations of the blood and vessels, such conditions as are produced in the neck by gentle pressure with a stethoscope or by tightened integument. This sound is continuous, and several of the patients who presented it had suffered from hæmoptysis. Crumpling sounds had been frequently heard. A great means of discovering phthisis was afforded in the differences in the character and amount of respiration; and he (Dr. Alison) believed that the instrument which he had made, which gave a stethoscope for each ear, and which he designated the Differential Stethoscope, would prove available in rendering very slight differences in respiration appreciable, which could scarcely be discovered by the ordinary stethoscope. The ordinary stethoscope necessitated removal of the instrument from one part to another, and a certain loss of time, though slight in itself, important when comparing two sensations nearly alike, was incurred. For the diagnosis of pulmonary consumption at its commencement, we should look for the signs of that disease at that period, and not for those of later periods. The acoustic properties of the lung with small points or spots of tubercle were, and must be, different from those of that lung which is so studded with tubercles, or so infiltrated with that material, that nearly all the lung-tissue proper is pressed upon or obliterated, or when the lung is broken down and has little cohesion, and presents numerous cavities. (The Differential Stethoscope was exhibited to the society.) In many examples of pulmonary consumption no dulness on percussion whatever is found; and not one of the recognised signs is present in all cases, or even at all times in the same case. We must be content with a certain amount of evidence, and that will not be the same in all cases, or in

the same case at different times. Deviations from the natural configuration of the chest occurred in pulmonary consumption at an early period. These were made out by their history and by comparing one side with another. The author's Chest-Goniometer would serve in discovering the deviations from the natural angles and curves, and in measuring them. The measurement at one period might be compared with the measurement at another.

Specimens of tuberculated lung, both in the early and later stages of the disease, served to prove that the physical signs in the different conditions of lung must greatly vary, and that the lung dotted with solitary tubercles the size of mustard-seeds would afford few, if any, of the ordinary signs, and chiefly produce deviations in quality from the natural respiratory sounds, and some such delicate new sounds as had been referred to. The author was not prepared to say that these delicate sounds would not be found in other morbid states besides phthisis, but the same limitation held in respect of all other sounds.

The examination of the sputum, and the discovery, by means of the microscope, of tubercle and lung-tissue, were referred to.

ART. 54.—*On the influence of sea-voyages and warm countries upon the course of Phthisis.* By Dr. J. ROCHARD, Surgeon-in-Chief of the Marine at Brest.

(*Archiv. Gén. de Méd.*, Feb., 1858.)

This memoir, crowned by the Academy of Medicine in Paris, is calculated to shake our faith in the ordinarily received opinion that sea-voyages and warm countries exercise a favorable influence upon the course of pulmonary consumption. Dr. Rochard has found, it appears, that consumption is more common in the navy than in the army, and that the course of this disease, and the development of tuberculization, is more rapid on the sea than it is on land. Hence, the naval profession is to be avoided where there is a predisposition to consumption. Dr. Rochard also advances some statistical evidence for supposing that the tropics and all hot countries are particularly obnoxious to consumptive patients.

ART. 55.—*On the supposed proclivity to Phthisis in persons of xanthous complexion.* By Dr. JOHN BEDDOE, of Clifton.

(*Edin. Med. Journal*, July, 1858.)

After some preliminary remarks on phthisical patients, Dr. Beddoe proceeds to his own observations—observations which derive their value from the fact that certain investigations, undertaken for another purpose, into the proportions in which the same combinations of complexional colours occur in the general population, enable him to furnish a trustworthy standard of comparison. The number of cases noted was 500; of the patients, 240 were natives of Scotland, and were almost all observed in the Royal Infirmary of Edinburgh; 217 were English, of whom 74 were seen in Brompton Hospital, and the rest in London, Bristol, and various parts of the country; 43 were Irish, seen in Edinburgh and elsewhere. Persons having gray hair were not noted.

The following tables exhibit the complexional characters of these 500 persons, compared with those of the general population of London, Bristol, and Edinburgh, of the natives of Edinburgh admitted into the Infirmary for all diseases, and of the Irish population of that city. The proportions are in all cases reduced to per-centages :

TABLE II.

	No. of Observations.	Eyes Light.					Eyes Neutral.					Eyes Dark.				
		Hair.					Hair.					Hair.				
		Red.	Fair.	Brn.	Dark.	Black.	Red.	Fair.	Brn.	Dark.	Black.	Red.	Fair.	Brn.	Dark.	Black.
Phthisical Individuals, Of whom Scotch, Natives of Edinburgh, Royal Infirmary, Edinburgh Streets, London Streets, Bristol, Streets, Irish, mostly Ulster and Connaught, }	500	3·8	9·9	21·2	13·9	1·7	·8	1·5	4·3	7·5	·6	1·1	·7	5·9	20·6	7·
	240	4·4	9·6	1·5	12·3	3·1	·8	2·7	3·1	4·8	·2	1·7	·4	7·5	21·7	6·2
	250	4·6	11·	25·2	10·6	1·	·4	2·4	8·	5·2	·4	·6	1·	7·6	16·8	5·2
	2500	5·2	13·8	28·	11·2	1·2	·6	1·8	5·7	5·3	·7	·7	1·1	6·4	13·7	4·5
	2000	3·1	9·4	28·5	11·9	·5	·5	1·7	6·8	5·5	·3	·6	1·2	7·2	19·8	3·
	1000	2·	10·8	29·7	13·1	·6	·5	1·3	5·1	5·	·2	·9	·5	6·7	20·2	3·3
	575	5·2	9·1	30·9	17·1	8·7	·2	1·	3·7	6·6	2·1	·2	·2	2·6	11·	6·6

Perhaps the facts may be rendered more easily appreciable by a different arrangement, as in Table III.

TABLE III.

Eyes.				Hair.				
Light	Neutral	Dark		Red	Fair	Brown	Dk. brown	Blk. *
50·0	14·7	35·3	Phthisical Persons,	5·2	12·1	31·4	42·	9·3
50·9	11·6	37·5	Do. Scotch,	6·9	12·7	32·1	38·8	9·5
52·4	16·4	31·2	Edinburgh Natives,	5·6	14·4	40·8	32·6	6·6
59·4	14·1	26·4	Edinburgh Streets,	6·5	16·7	40·1	30·8	6·4
53·4	14·8	31·8	London . .	4·2	12·3	42·5	37·2	3·8
56·2	12·1	31·6	Bristol . .	3·4	12·6	41·5	38·3	4·1
66·	13·6	20·6	Ireland . .	5·6	10·3	37·	34·7	12·4

* This class includes not only coal-black, which is rare in this country, but certain shades of dark brown, which are not readily discriminated from black unless in a very good light.

The indications of these tables seem to me tolerably clear. They not only disprove the old notion of the special liability of xanthous persons, but go far towards proving consumption to be more rife among dark-eyed, dark-haired people.

Dark eyes, it will be seen, were more frequently met with among the phthysical than in any one of the five sections of the general population, with which Dr. Beddoe has compared them.

Red hair rose slightly above, and fair hair fell a little below the average. Brown hair (corresponding to the "chatain" and "chatain clair" of the French, and not to their "brun") counted little more than three fourths of its proportionate number. Dark brown, on the other hand, rose almost as high above the average, and black still higher, showing an excess in the proportion of more than 3 to 2. The frequency of black hair among the non-phthysical Irish does not tell much on the average, as Ireland furnished only 43 cases out of his 500—less than 9 per cent.

Dr. Beddoe could detect only one possible source of fallacy worth mentioning. It is conceivable that the progress of the disease may be in general more rapid, and the duration of sojourn in hospital consequently shorter, in fair than in dark subjects. The result of this would be to diminish unduly the number of xanthous persons present in hospital at any given time. The observations taken in Edinburgh, fully one-half of the whole, are almost free from this objection, having been made while the author was resident in the infirmary, and had the opportunity of examining the cases on or soon after their admission.

That consumption may be very frequent among persons of fine skin and delicate complexion, Dr. Beddoe by no means intends to deny; in fact, he believes that a very fair complexion, especially when conjoined with black hair and eyes, is very often associated with proclivity to tubercular disease. But into this question he does not enter, inasmuch as it cannot, like that which has just been discussed, be readily brought to the touchstone of numbers.

ART. 56.—*Effect of local influence on spasmodic Asthma.* By Dr. HYDE SALTER, Assistant-Physician to the Charing Cross Hospital.

(*Edin. Med. Journal*, June, 1858.)

The following conclusions appear to be established by several cases related in this paper:

1. That residence in one locality will cure, radically and permanently cure, asthma resisting all treatment in another locality.
2. That the localities that are the most beneficial to the largest number of cases are large, populous, and smoky cities.
3. That this effect of locality depends probably on the air.
4. That the worse the air for the general health, the better, as a rule, for asthma; thus the worst part of cities are the best, and conversely.
5. That this is not always the case, the very reverse being sometimes so—a city-air not being tolerated, and an open pure air effecting a cure.
6. That there is no end of the apparent caprice of asthma in this respect, the most varying and opposite airs unaccountably curing.

7. That, consequently, it is impossible to predict what will be the effect of any given air, but that probably the most opposite to that in which the asthma seems worst, will cure.

8. That some of these differences determining the presence of cure of asthma appear to be of the slightest possible kind, arbitrary and inscrutable.

9. That the mere conditions of locality appear to be adequate to the production of asthma, in a person whose disposition to it was never before suspected, and who probably would never have had it had he not gone to such a locality.

10. That, consequently, many healthy persons, who never have had asthma, and never may, probably would be asthmatics if their life had been cast into other localities.

11. That *possibly* there is no case of asthma that might not be cured if the right air could only be found.

12. That the disposition is not eradicated, merely suspended, and immediately shows itself on a recurrence to the original injurious air.

13. That change of air, as change, is prejudicial.

14. That, from the caprice of asthma, the constancy of the results in any given case is often deranged.

ART. 57.—*On measuring the capacity of the Chest in disease.* By Dr. J. SCOTT ALISON, Assistant-Physician to the Hospital for Consumption, Brompton.

(*Dr. Beale's Archiv. of Med.*, No. II, 1858.)

While the capacity, the dimensions, and the expansion of the thorax are respectively well gauged by the spirometer, the inch-measure, the callipers, and the stethometers of Quain and Sibson, we have hitherto employed no means for measuring the form of the chest as represented by its curves and the angles at which the planes of its component parts meet. To measure the curves and angles of the chest the above means are totally void. It frequently happens that the natural curve of a part of the thorax becomes altered in disease, that a curve is replaced by an angle, and that one angle is substituted for another. The eye, it is true, may detect these changes, but by measurement exactitude is obtained, and a record is formed which, at another time, may serve for comparison and as a test of reduction or increment. For the measurement of curves and angles the stetho-goniometer has been constructed, and its employment has been found useful. By means of it important alterations have been gauged. Certain deviations from the natural configuration of the chest, of great extent, as shown by this instrument, have been measured which elude the operation of all other means. A loss or increase of roundness in the upper and front part of the thorax, so small as to defy the callipers and inch-measure, will be made manifest and be exactly measured by the stetho-goniometer. If, as often happens in phthisis, the articulation of the second costal cartilage with the sternum, or of the second rib with its cartilage, instead of forming part of a curve, has become angular, the deviation is accurately measured.

It has been found that at a very early period of many examples of pulmonary consumption, one side of the chest undergoes an alteration in its curves, and differs from the other side; the stetho-goniometer accurately measures this loss of symmetry. The stetho-goniometer may be used not only to measure the angles which the plane of one portion of the chest makes with that of another part, but to measure the angle at which a part of the chest meets the horizontal or the perpendicular line. Thus, the first rib may, from contraction of the lungs and pleura, decline as it proceeds from the median line of the body; the amount of this declination may be accurately learned by placing one arm of the stetho-goniometer along the plane of the rib and the other in the plane of the horizon. The angle shown on the instrument is the angle of the rib with the horizontal line. The plane of a part may be compared with the median or transverse lines of the body. The sternum may be deflected from its median position assuming an oblique situation; the angle it forms with the median line is shown by placing one arm of the instrument in the plane of the sternum and the other in the median line. The exact amount of deformity is thus ascertained.

The stetho-goniometer is constructed of ivory. It is composed of two arms, the same as the goniometer employed for the measurement of crystals. The arms are three inches long, and are jointed together. An arc, graduated into degrees, is attached to the arms where they meet and at their revolving point. A vernier is placed upon the arc for the purpose of measuring a part of a degree (1°). This vernier subdivides the degree (1°) into twelve parts, and each of these twelve parts represents $5'$ (minutes). The vernier will seldom be required in measuring the chest, as *extreme* nicety is not commonly desiderated. The arrow upon the vernier-arm is the index of the degree. When both arms of the stetho-goniometer are in the same plane, as upon a level surface, the arrow on the vernier-arm points to 180° upon the arc. The degrees upon the arc range from 20° to 220° , which will include all angles which will usually come under treatment.

When an angular part is to be measured, the centre or junction part of the instrument is applied upon the point of junction of the two planes, and the arms are respectively placed upon them, the edge of the arms being set upon the parts to be measured. The arrow now indicates the degree.

When a curve is to be measured, say the curve of the natural mammary region, or the lateral curve of the dorsal spine, the part of the curve which is its apex is fixed upon, and the centre part of the instrument is placed over it, while the arms are made to touch respectively a point in the middle of the part of the curve on either side of the apex. The arrow on the vernier points on the arc to the degree of the angle to the tangents to the curve. Curves belonging to greater or smaller circles may be thus advantageously compared.

Depressions or hollow parts of the chest may be measured with the stetho-goniometer. When angular, the instrument, at its joint, is applied to the point of union of the two planes of the part, and the two arms laid upon the retiring planes respectively. When a curved hollow is to be measured, the centre part of the instrument is placed near the lowest part of the hollow or depression, and, as it were, opposite to the apex of

the curve, and the arms are held on either side, parallel with two imaginary tangential lines.

The deviations which most frequently occur and which it is desirable to measure, are chiefly the products of tubercle, of cavities in the lung, pleurisy, and the obliteration of the vesicular structure of the lung which occurs in pneumonia and bronchitis, also of empyema and intra-thoracic tumours.

The stetho-goniometer is light, portable, simple, and exact, and easy of application.

(C) CONCERNING THE CIRCULATORY SYSTEM.

ART. 58.—*On the results of adherent Pericardium.*

By Dr. GAIRDNER, Physician to the Edinburgh Infirmary.

(*Edinburgh Medical Journal*, June, 1858.)

The object of this note is to direct attention to some statements of facts brought forward by Mr. Henry Kennedy, and which do not harmonise with certain conclusions arrived at by Dr. Gairdner in 1851.

"The only thing in Mr. Kennedy's paper which can be called new," writes Dr. Gairdner, "is a collection of 90 cases of adherent pericardium, 'excluding strictly all where valvular disease was present.' From this large assemblage of cases he hopes to prove the proportion in which hypertrophy, dilatation, and atrophy follow upon adhesion. Unfortunately, he omits to tell us whence the cases are derived; though it seems scarcely too much to infer that they are collected from 'museums and catalogues,' either in Dublin or elsewhere. In a note the author expresses a misgiving, which I believe to be but too well founded, as to the inadequacy of his data to bear the conclusions which he rests upon them.

"These conclusions are as follows (I give them the form of general expressions, in order to compare them with my own): 1st, that in simply adherent pericardium the heart remains healthy till death in not much more than one third of the cases (34 out of 90); 2dly, that it undergoes hypertrophy, or hypertrophy with dilatation, in considerably more than a half (51 out of 90); 3dly, that it undergoes atrophy in one eighteenth of the cases.

"It is in reference to this last conclusion that the author chiefly expresses the misgiving referred to above. 'Can it be,' he says, 'that specimens of this state *are not kept in our museums or catalogues*; hypertrophy and dilated cavities only being *thought worthy of putting up?*' I shall not presume to judge absolutely whether this be so; but that some further explanation is required, not only of this, but of the other conclusions above mentioned, I firmly believe; and this on the following grounds, derived from the paper in the '*Monthly Journal*' for February, 1851.

"From a series of 500 miscellaneous post-mortem examinations, performed in the Edinburgh Infirmary, I carefully selected all the cases of adherent pericardium in which the adhesions were so considerable, and

so situated, as to restrain the movements of the heart. It is probable that Mr. Kennedy, to obtain a similarly broad basis for his 90 cases of adhesion, would have required to search through the records of 3000 general cases; for my 500 cases only yielded 15 such adhesions. Of these 15, I found 5 in which the condition of the heart was morbid; 10 in which it was not so. Add one, or even two more, as being *possibly* within the limits of disease; it will still be true, that in more than a half of these cases the heart had suffered no apparent morbid change, although in all of them the adhesions were plainly of very long standing. But as this statement includes two cases of valvular and other disease of the heart, excluded, and rightly excluded, by Mr. Kennedy's plan (though retained by me for reasons specially stated), it would be nearer the truth to say, that in at least two thirds of my cases of adherent pericardium there was no secondary lesion of the heart fairly attributable to the existence of the adhesions.

"The case, therefore, as between Mr. Kennedy's results and my own, in regard to the existence of secondary disease of the heart, stands thus: Mr. Kennedy finds secondary disease in nearly two thirds of his cases; I find it in about one third of mine. In his, in other words, the large majority were morbid; in mine, the large majority were either healthy, or not decidedly morbid. Is this the consequence of the selection on his part being from 'museums and catalogues,' instead of from the whole field of nature? I am disposed to think so.

"As regards the question of hypertrophy and atrophy, I am quite sure, from my own observation, that there is a good deal of room for difference between different observers, according to their preconceived ideas of what conditions are entitled to these names. I have very frequently seen very small-sized hearts in connexion with adhesions of the pericardium; several such cases, indeed, are alluded to in my paper in the 'Monthly Journal.' But in all of these the small size of the heart appeared to me, at the time, fully accounted for by the state of the general system. If it really deserved to be called atrophy in these cases, it was part of a general atrophy of the muscular system, and no special cardiac disease. I have, indeed, seen a very few cases which have appeared to justify the opinion of Dr. Chevers and others, that adherent pericardium may lead directly to atrophy, when the adhesions are very dense; and especially, I would say, when they have supervened upon very long-continued effusion. But such cases are, I believe, quite exceptional; and I am fully satisfied that ordinary fibrous adhesions, when not so thick and dense as altogether to prevent expansion mechanically, tend rather in the direction of hypertrophy and dilatation than of atrophy.

"On the whole, I am very willing that the doubtful points in this difficult subject should be reserved for further and more accurate observation; but I can see no reason, in the meantime, to abandon the conclusions which I was led to adopt in 1851. I have little difficulty, on the one hand, in rejecting the opinion of Hope and others as to the invariably and rapidly fatal tendency of pericardial adhesions; on the other hand, I regard them as a cause of disturbance very likely to precipitate the course of other diseases leading to embarrassment of the circulation, and not unlikely to be followed, after a longer or shorter time, by hypertrophy and dilatation. For the fuller statement of these opinions I beg

to refer to the paper itself; commending the whole subject to the attention of the clinical observer, by whom alone satisfactory data as to the course of this disease can ultimately be furnished. In the meantime, let us not, upon too light grounds, deprive ourselves of the modicum of comfort and satisfaction which we may derive from believing that a certain, not small, proportion of persons affected with pericarditis, and with its sequela of adhesion, may survive the attack for many years, and may live in tolerable comfort, under favorable circumstances, without the inconvenience and the dangers attendant upon a dilated, hypertrophied, or atrophied heart. I believe that I have seen such cases; though from their very nature their diagnosis must be uncertain, as their ultimate issue must remain doubtful."

ART. 59.—*Acute fatty degeneration of the Heart as a complication of pericarditis.* By Professor VIRCHOW.

(*Archiv f. Pathol. Anat.*, t. xiii, 1857; and *Archiv. Générales de Méd.*, Sept. 1858.)

M. Virchow has met with two instances of this change, one in a person who had purulent pericarditis and died suddenly; the other, in a person who had been attacked with hæmorrhagic pericarditis and died speedily, feebleness of the pulse soon followed by intermittency being the most marked phenomena. In both these cases the superficial muscular layers of the heart were yellowish, opaque, and friable, and so far advanced in the state of fatty degeneration that their proper muscular structure was no longer discernible; and in a lesser degree every part of the heart was affected in the same manner. This form of fatty degeneration differs essentially from the ordinary form, which pursues a chronic course, and affects, first, the muscular fibres subjacent to the endocardium. It is considered by M. Virchow as the direct effect of the pericarditis—an effect of the extension of the inflammation of the serous membrane inwards.

ART. 60.—*On the relative importance of disease of the Aortic and Mitral Valves.* By Dr. WILKS, Assistant-Physician to Guy's Hospital.

(*Guy's Hospital Reports*, 3d series, vol. iv, 1858.)

"In looking through our cases of heart-disease, and observing the histories accompanying them, we think we discern the reason for the difference of opinion entertained respecting the duration and relative importance of the two forms above mentioned. Judging simply from clinical experience, we should incline to second the opinion generally held respecting them, that the mitral is the more severe disease, that is, that when the patient with this form of malady comes before us he is very often (at least in hospital practice) suffering from dropsy and other symptoms denoting speedy dissolution, whereas the patient with aortic disease speaks of symptoms which have had longer duration and less severity, and he perhaps leaves us again in improved health. Judging then from the duration of illness or loss of health in the two cases, we conclude that the general opinion is correct as to the greater severity of the disease which has its origin in the mitral valve. If, however,

we endeavour to discover the time at which the respective maladies commenced, we may readily arrive at an opposite conclusion, but then we are obliged to adopt a different method in the two cases. In the first place, we must inquire what is the origin of the two forms of disease. Our own records most fully corroborate the opinion that disease of the left auriculo-ventricular orifice has its origin in rheumatic endocarditis, and that the disease of the aortic orifice is due generally to a strain on the vessel or valves, and occurs for the most part in men who are accustomed to work hard and use strenuous exertions with their arms. Disease of the aortic valves undoubtedly may arise from endocarditis, but in the majority of instances it appears to arise from the cause named; but whether this be from undue pressure acting on the vessel through the parietes of the chest, or whether from an over-strain of the blood on the vessel internally, is a question not yet decided. We have then two classes of cases (of course with numerous exceptions) of valvular disease, that of the mitral and that of the aortic valves, and since the former so generally arises during a rheumatic attack we generally date its origin to the time at which that occurred, whereas, in the latter case, we can do nothing but discover the duration of the symptoms, and date the commencement of the disease to their first recognition. In one case we inquire about the rheumatic attack, and in the other, how long symptoms denoting cardiac disease have existed. In the majority of the latter cases, which are those of men who are sawyers, deal-porters, &c., and accustomed to great exertions, we find that the symptoms have existed only for two or three years before death; in some cases the time is longer, but this appears to be the most usual period for the progress of the malady; whereas, in the case of disease of the auriculo-ventricular orifice, although we may not have a history of marked symptoms for so long a period, yet the patient may allude to some slight distress connected with the circulation for a period dating from a rheumatic attack perhaps twenty years before, though in many cases there have been no symptoms whatever. Judging from a large number of cases, we think the conclusion is correct, that the disease of the mitral valve has been for the most part of longer standing than that of the aortic; but then the question at once arises, because an inflammatory process was set up at the period supposed or even a contraction of the orifice then took place, does it necessarily follow that such an impairment of the apparatus occurred as to constitute it a disease, or make it productive of manifest symptoms which might be styled unequivocally cardiac. We think in all probability in these cases of contracted mitral orifice no great impairment of the orifice took place, seeing that no marked symptoms had been present, and we cannot even look upon the changes which may have occurred in the cavities of the heart as an indication of the duration of its existence. The enlarged and hypertrophied left auricle, and increase of right side of heart, does not explain how long the contracted orifice has existed, any more than the enlarged left ventricle does the aortic mischief. This difficulty in knowing how long a contraction of the mitral valve has existed, recurs in every case met with; we date the first onset to the rheumatic attack, even should this have occurred several years previously; but then the question arises, did the contraction altogether oc-

cur at that time, but that owing to subsequent conservative changes in the heart and generally diminished circulation, of which the mitral orifice is the measure, no symptoms of cardiac disease existed, or has the change in the valve been essentially chronic and progressing during several years, or was the alteration in the first place slight, and subsequently during the time of the last fatal illness the more important changes occur; whichever view may be taken, there can be no doubt that very important changes do take place during this latter time, such as chalky degeneration, alterations in the muscular tissue, &c., which necessitate regurgitation through the orifice and the more urgent symptoms.

"In commencing to make these remarks upon this question we had intended to take the duration of the disease in the two classes of cases, and striking an average, show the difference between them. This, however, we shall not do, as it might lead to error, for reasons above named, and also because the history attached to the *post-mortem* records has been derived from other sources than our own, and the dates of the illnesses cannot be accurately depended upon; still, on looking through a large number of cases on which we can rely, the inference is as is stated—that, taking the duration of the severe symptoms, or the time in which the patient has called himself ill, the disease of the aortic valves has continued for a longer time, that is two or three years, while that of the mitral only for a few months; and, on the other hand, as in the one case we have no knowledge of the existence of the disease otherwise than connected with symptoms, and we only date it back to their commencement, whereas, in the case of mitral disease we date it to an inflammatory attack which has generally occurred several years before, we find the opposite rule holds good, that disease of the mitral valve is of much longer duration before a fatal issue than disease of the aortic valves.

"These remarks we think explain the different opinions which we have heard expressed respecting the relative severity and importance of the two lesions, but they do not solve the question; this we think can only be done by carefully watching several cases during many years. If, for example, in the two classes of cases, the time of onset of disease could be accurately known, and the cases watched to their conclusion, some approach to accuracy might be obtained, though even then it is possible that slight changes might have existed for years without symptoms. We believe most physicians would admit that a regurgitation through the mitral orifice was more important than a similar condition of the aortic; but it is another matter how long the two affections may have been in process of production, and although the one may be a more important lesion than the other, and the disease more speedy when such lesion obtains, yet the process preceding this may have been of much longer standing, being in fact a slower change, the result of inflammation unattended by symptoms, while the other disease might be called accidental, and at once productive of slight disturbance. This we believe to be a fair conclusion from the result of our cases, not very precise we admit, but we are unwilling to add any greater weight to our statements by the addition of figures, our object being at present merely to afford an explanation of the different opinions which have been expressed on this subject."

ART. 61.—*On some peculiar Cardiac Sounds.* By Dr. EDWARD SMITH, Assistant-Physician to the Hospital for Consumption at Brompton.

(*Lancet*, Sept. 11, 1858.)

Three patients were recently under Dr. Smith's care, in whom he has ascertained the existence of a sound in the chest which is rare and deserving of notice. It is situate only in the second and third intercostal spaces of the left side, on front, and midway between mid-sternum and coracoid process, and is restricted to a space which may be covered by the stethoscope. The following is a short description of each case, with notes of the character of the sound made at each visit :

CASE 1.—M. N—, a female, æt. 21 ; single ; a servant ; has tubercle of both lungs in a state of softening. She has been ill but two months, and then had giddiness and a little hæmoptysis, followed by a little cough, but no dyspnœa, or palpitation of the heart. Soon afterwards, and not before, she lost flesh and strength, and had swelling of the feet. She had also pain on the level of the left nipple, which passed up to the left shoulder. She is now thin, very excitable, and has flushed cheeks, with much debility and dyspnœa on exertion. There is but little cough or spitting. The appetite and digestion are good. Tongue clean, and bowels regular. The pulse is 136, and respiration 34 per minute ; and the vital capacity is only 50 cubic inches. Her height is 5 ft. 0 $\frac{1}{4}$ in. On the right side there is great dulness on percussion universally, with very feeble and short inspiratory efforts, and a cavity. On the left side, the dulness is chiefly below the third rib, with harsh respiration and indistinct signs of a cavity at the third intercostal space, stretching across the chest in the left side. At this place there is a short, sharp thrill with each pulsation of the heart, and there are three between the inspirations. It is not heard during inspiration, because the inspiratory sound has a similar tone and quality, and is louder ; but it is heard immediately the inspiration has ended, and with the expiration, and in the interval between the inspirations. It is heard when the breath is held in expiration ; but it is then of a softer quality, and seems a little more distant. It is not heard when the breath is held in inspiration. There is a double sound at the base of the heart ; but at the apex the sounds are normal. Both pulses are synchronous with the sound under discussion, and are distinct and regular. There is no thrill when the hand is placed over the heart or the lungs, and no peculiar movement in the arteries or veins ; but the action of the heart feels tumultuous. She never had pain at this spot, nor any sign but palpitation, and that during a fortnight only.

In a fortnight the sound was still audible at the same place, but somewhat less extensive. The respiratory sounds are still harsh, short, and wavy. She is no worse. In ten days further the sound is still distinctly heard, but it is not heard when she whispers.

Dr. Smith was of opinion that this sound was a modified heart-sound ; and the sound of the heart could be distinctly heard at the same time in another tone. The quality of the tone of the sound referred to is much that of a rough bronchial respiration.

CASE 2.—C. H—, a shipwright, æt. 25 ; single ; has tubercle with a cavity in the right lung, and probably deposited tubercle in the left. His vital capacity is 152 cubic inches. On April 30th the heart was healthy. On May 14th he complained of pains in the left side with palpitation ; but, except for a foul state of the tongue, he progressed very favorably through

the whole year. On December 24th, Dr. Smith discovered the sound referred to in the third space, and then learnt that sometimes he had suffered from pain at that spot. There is a systolic bruit now perceptible at the apex of the heart, and it appears that he had rheumatism badly thirteen years ago. The pulse is 90 per minute, whilst the repetition of the sound referred to is 124 per minute. On February 3d there was a rough first sound of the heart, and the sound under inquiry was still heard in the second and third spaces. He was discharged, feeling almost well.

CASE 3.—The third case is that of a smith, æt. 20, single, and very pale, thin, and feeble, who two months previously appears to have had an attack of pneumonia on the left side, but he was not at that time seriously ill. He has much cough and dyspnœa and frothy sputa, and his tongue is white and bloodless. There is tubercular deposition and a cavity in the right side, and in the left much consolidation, but chiefly at the base, and there is wavy respiration. The sound under discussion is heard in the second space on the left side, chiefly in the position before mentioned. The sounds of the heart are heard under the stethoscope fully one inch to the right of the sternum, and do not quite extend to the nipple; so that there is possibly some displacement of the heart, but certainly enlargement of the heart on its right side. In five weeks the sound is still audible, but not with constancy; and it is not heard with full inspiration, nor without respiration. The pulse is 140, and the respiration 21 per minute.

ART. 62.—*A Needle implanted in the Septum of the Heart without causing special symptoms.* By M. PIORRY.

(*L'Union Médicale*, March 18, 1858; and *Med.-Chir. Review*, July, 1858.)

A man, æt. 54, a drunkard, was admitted into the Charité on February 10th, 1858, with symptoms of pneumonia. He soon recovered, and the examination of the heart during his illness showed no abnormal bruits or special symptoms. Erysipelas of the face supervened, and death ensued on the 20th of February. The diseased lung was gorged with frothy mucus, the trachea full of sputa, the right side of the heart was dilated, and the liver very voluminous. In examining the heart a hard substance was felt, which proved to be a needle, one millimetre in width, and five centimetres at least long. It was inserted in the interventricular septum; its ends were free, the point being directed towards the left ventricle. M. Piorry is of opinion that the needle was introduced through the intercostal spaces, that it was gradually drawn away from the surface by the movements of the heart, and thus became fixed in the septum. A fibrinous concretion was found on the ventricular pericardium where the needle had entered, and both ends of the needle were covered by a fibrinous layer.

ART. 63.—*An Hydatid Tumour in the apex of the right ventricle of the Heart, and free hydatids in the right pulmonary artery.* By Dr. BUDD, Professor of Medicine in King's College, London.

(*Medical Times and Gazette*, July 17, 1858.)

An hydatid tumour developed in the muscular substance of the heart is of such rare occurrence that the following case deserves to be placed on record.

CASE.—Sarah Sheppard, a single woman, 23 years of age, stout and florid, was admitted into King's College Hospital on the 23d of December, 1857. For the nine months preceding she had been engaged in millinery, but previously was in service.

She stated that four years ago she was laid up with pleurisy and inflammation of the kidneys. (Great pain in the loins, and dark-coloured muddy urine were symptoms of this latter affection, but there was no dropsy.) From that time she had been constantly troubled more or less with cough, shortness of breath, and palpitation. Two years ago she had another attack of pleurisy. Since this second illness her health had declined, and her cough had been attended with expectoration, the matter of which was often streaked with blood.

Nine days before she entered the hospital she "took cold," and swelling of the legs came on.

On her admission to the hospital she complained of cough and shortness of breath, and her feet were slightly cedematous. While lying still in bed, she had no pain or urgent symptoms, but slight exertion caused considerable dyspnœa. With her cough, which was very troublesome, she spat up mucus, partly clear and partly opaque, streaked here and there with blood. On listening to the chest, a systolic rasp-sound was heard over the base of the heart, and extending thence a little upwards and to the right. The impulse of the heart was slight, and the pulse very small and feeble. The tongue was coated, and the appetite bad. Menstruation was regular. The urine was of specific gravity 1020, turbid, with lithic deposit, and contained a very small quantity of albumen.

On the 28th of December, it was noted that the abnormal systolic heart-sound was much less rough; and on the 30th, that no morbid bruit could be heard. From that time till the poor woman's death, though I often listened for that purpose, I never heard any distinct morbid sound with the heart's beats; but the physician's assistant, who lived in the hospital, and examined her still more frequently, told me that he occasionally heard a faint systolic bellows-murmur.

From the 23d of December—the day of her admission—to the 9th of January, there was no other noteworthy change in Sheppard's condition. The cough was very troublesome, and the matter expectorated was constantly streaked with blood. The pulse ranged from 90 to 100; the number of inspirations from 36 to 48 in the minute. Crepitation was heard over both lungs behind.

On the 9th of January she spat up nearly half a pint of blood, mixed with viscid mucus; and for some days afterwards the pulse and inspirations were less frequent, the cough was less harassing, and the breathing somewhat easier.

On the 27th of January she spat blood again, in less amount.

It was noted on the 29th of January, that she keeps up her strength, and does not lose flesh. From this time she continued much in the same condition—distressed by difficulty of breathing and by cough, and spitting up mucus, generally tinged with blood. The difficulty of breathing varied considerably on different days. In the space of a fortnight—from the 26th of January to the 9th of February—the number of inspirations ranged from 30 to 48. The pulse was constantly small, but its rate varied in the same time from 72 to 90.

On the 19th of February it was noted that the breathing at the base of the lungs was nearly clear, and on the 24th of February she left the hospital.

On the 28th of February she again spat a considerable quantity of blood,

and her distress of breathing increasing, she was taken into the hospital again on the 3d of March.

It was then remarked that there was a rough respiratory murmur over the upper part of the left lung in front, and over the lower lobe of the right lung behind.

On the 14th of March, œdema of the legs, which had disappeared for some time, came on again. The urine then contained no albumen.

On the 7th of April she became affected with sore-throat, and a deep ulcer formed on the left tonsil. The soreness of the throat ceased in ten days or a fortnight.

On the 12th of April she complained much of pain shooting through the left side of the chest.

From this time she often complained of intense pain in the præcordia. The præcordial space, dull on percussion, was unusually extensive; but no unnatural bellows-murmur was heard. The impulse of the heart was tolerably strong, and its action throughout was regular. The sounds of bronchitis were heard over the upper part of the left lung in front, and over both lungs behind; but in no part of the chest were respiratory sounds altogether absent. The dropsical swelling of the legs increased, and ascites also came on. The distress of breathing amounted at times to extreme orthopnoea; and the countenance, which had throughout a purplish tint, was expressive of great distress. The legs and thighs and abdomen became at length tensely œdematous.

On the afternoon of the 4th of May, Dr. Duffin, the physician's assistant, on being summoned to her, found her pale, gasping at long intervals, and with a scarcely perceptible pulse. About five minutes afterwards she died.

On examination of the body both lungs were found united to the pleura costalis by old adhesions. The pericardium contained about an ounce of serous fluid. Its layers posteriorly were glued together by tolerably old adhesions. The heart was of a very irregular shape, flattened anteriorly, and bulging posteriorly. Its irregular shape was owing to an hydatid tumour, about the size of an orange, situated in the apex of the right ventricle, and projecting into its cavity. The right auricle and ventricle were filled with clotted blood; the left chambers of the heart were empty. There was no disease of the valves.

Under one of the laminæ of the tricuspid valve a small flaccid hydatid was found, unattached. In the pulmonary artery, immediately above the valves, an unbroken hydatid, rather more than half an inch in diameter, was found; and in the further course of the artery, before its subdivision, there were several other smaller hydatids.

On tracing the branches of the pulmonary artery, several clusters of hydatids and the collapsed skins of hydatids—ranging from one eighth to one fourth of an inch in diameter—were discovered in them. These hydatids were exclusively confined to the left lung, and chiefly to the upper lobe, one small cluster only being found in the centre of the lung, and one in the lower lobe. These clusters of hydatids were enveloped in pale fibrin, but not contained in organized sacs. The lower lobes of both lungs were carnified, but still crepitated slightly under the fingers. The pulmonary veins and the bronchial tubes contained no hydatids.

The liver, spleen, kidneys, stomach, uterus, and brain, and the principal venous trunks of the lower extremities were next carefully examined; but, with the exception of slight fatty degeneration of the liver, some irregularity of the surface of the right kidney, and general venous congestion, nothing abnormal was detected.

On examining one of the small hydatids taken from the pulmonary artery, I found it to contain very perfect echinococci.

The hydatid tumour in the apex of the heart was stuffed with hydatids, and it was evident that the hydatids found in the right ventricle and in the pulmonary artery had escaped from it.

A review of the course of Sheppard's illness leaves little doubt that the hydatid tumour had existed in the heart for several years. Four years before her death she was laid up with pleurisy and what was termed inflammation of the kidneys, and ever afterwards was troubled with cough, shortness of breath, and palpitation. Two years before her death she had a second attack of pleurisy, and subsequently to this frequent spitting of blood. After her death, old adhesions of the lung to the pleura costalis—such as would have resulted from attacks of pleurisy at the dates specified—were found.

Now, the poor woman was of vigorous conformation, and to the last was stout and florid. There can, therefore, be little doubt that these attacks of illness are attributable to the hydatids; but more than one other case may be cited to show that an hydatid tumour in the heart, provided it be unbroken, although it may cause terrible disturbance of the heart itself, ending in death, may not set up inflammation of the lung or pleura. It is, consequently, probable that the attacks of pleurisy were caused in Sheppard by the blood becoming contaminated by the hydatid liquor,—which, from the occasional bursting of an hydatid tumour into the sac of the peritoneum, we know to be highly irritating to serous membranes. In a case that fell under my care some years ago, in which an hydatid tumour in the liver opened through the lung, and a great number of broken hydatids were coughed up, inflammation of the pleura was excited by the passage of the hydatids, and after death the right lung was everywhere united to the reflected layer of the pleura.

Appended to this case are recorded a pair of similar cases.

(D) CONCERNING THE ALIMENTARY SYSTEM.

ART. 64.—*Contributions to the pathology of the Glandular Structures of the Stomach.* By Dr. WILSON FOX.

(*Proceedings of the Royal Med. and Chir. Society, June 22, 1858.*)

The observations recorded contain the result of a series of microscopical examinations of 100 stomachs, taken indiscriminately from the bodies brought for post-mortem examination to the Pathological Institute to the Charité Hospital, Berlin, under the direction of Professor Virchow. The principal morbid conditions noticed are classified under the heads of acute and chronic affections, and described under the title of "Catarrh of the Stomach." The microscopic characters of one case of acute gastritis are described; and mention is made of the appearances presented by a case of amyloid degeneration, and of one other specimen, presenting some unusual and hitherto undescribed changes, occurring apparently in the connective tissue intervening between the glands.

Acute catarrh, in addition to the naked-eye appearances of injection, swelling of the membrane, prominences caused by glands filled with epithelium, and an increased secretion of mucus, is characterised microscopically by an increased nutritive activity displayed by the epithelial

elements, which are produced with greater rapidity, and are of a larger size than normal, while their appearance is more granular than is seen in the healthy state. The glands, under these circumstances, have an unusually white appearance by reflected, and a darker look than natural by transmitted light, both of which, as well as the granular character of the individual cells, disappear on the addition of caustic alkalies. The cells break down with great facility, and frequently the gland-tubes are found filled with molecular *débris* and free nuclei. Slight fatty degeneration accompanies this condition, but does not proceed to any marked extent in the acute stages. "Granule-cells" have not been observed by the author. Both classes of glands, viz., those lined with a cylindrical, and those containing a spheroidal epithelium, appear to be equally affected, though the pyloric portion suffers with greater frequency than other parts of the stomach. The microscopic appearances observed in the case of acute gastritis recorded corresponded very closely to those above described, but the changes in the epithelial cells existed to a more marked degree, proportionate to the intense injection which was present.

The condition of *chronic catarrh*, which appears to result from repeated or long-continued attacks of the acute affection, is characterised by a series of changes analogous to those produced by chronic inflammation of other parts. It may be met with independently of any evidences of the acute affection, or appearances characteristic of both may appear simultaneously. The naked-eye appearances of chronic catarrh are—thickening of portions of the mucous membrane, occurring at times irregularly, and then giving rise to unevenness of the surface, which is due to prominence of individual follicles, and of groups of glands. Alterations in colour also occur: of these, the most characteristic is a slatey-gray discoloration, which is met with in patches of variable extent. Translucent spots are seen scattered over the surface, and in some parts dull opaque white patches. There is an increase of the mucous secretion, which frequently has a peculiar glassy look.

The microscopic changes are numerous, but may be classified under the heads of—

1. Increase in the amount of the connective tissue between the glands.
2. Thickening of the membrana limitans of the glands.
3. Fatty degeneration of the glandular epithelium, and atrophy, and loss of the epithelium of the tubes.
4. Pigmentary deposit in the tissue.
5. Cystic degeneration of the glands.

1. In estimating the amount of increase of the connective tissue a fallacy has to be avoided, which arises when sections are not made directly in the line of glands, when the amount of connective tissue appears greatly increased. The amount also varies in different parts, being greater in the pylorus and the immediate neighbourhood of the cardiac opening than in other parts. Cadaveric decomposition may also, by facilitating the destruction of the membrana limitans of the glands, give rise to an appearance under the microscope of nothing but fibrous tissue, in which free nuclei and molecular *débris* lie imbedded. A true increase is best estimated after the addition of acetic acid, when the interspaces

between the glands are seen to be widened, and the nuclei of the tissue are brought into view in increased numbers. The "état mamelonné" is occasionally produced by marked degrees of this condition, but it frequently occurs unassociated with any other morbid change, and cannot, therefore, be considered as pathognomonic of this or any other particular affection. An appearance of an independent development of free nuclei in the midst of the connective tissue, as described by Dr. Handfield Jones, has not been seen by the author, except under circumstances where there was suspicion of their having been introduced accidentally from the rupture of the glands. Increase of the connective tissue does not appear necessarily to cause atrophy of the gland-tubes, as the extensibility of the wall of the stomach obviates the consequences which the pressure produced by its retraction would cause in a less elastic organ.

2. Thickening of the *membrana limitans* has been observed as one of the appearances of this condition. In some, but not in all cases, the addition either of acetic acid or caustic alkalies may cause this membrane to swell, and thus give rise to a fallacy; but it has been observed by the author without the addition of reagents.

3. Fatty degeneration of the glandular epithelium is very frequent. Slight degrees of this change can scarcely be considered as morbid, but in many cases the interior of the glands is entirely filled with fat-drops.

4. Deposits of pigment may take place both in the epithelium and in the cells of the connective tissue. Its presence is due to the escape of hæmatin by rupture of the overloaded vessels during the congestion which attends the earlier stages.

5. Cystic degeneration of the glands is not unfrequently a consequence of the causes producing some of the above-mentioned conditions. The cysts may, on careful examination, be seen by the naked eye; and their origin, in changes in the glands, is proved by the fact that they contain either cylindrical or spheroidal epithelium, differing in this respect from the solitary glands of the intestine, whose contents are cells of smaller size and different characters. These are occasionally met with in the stomach; but the author believes that this frequency has been exaggerated, owing to the cystic degenerations having been mistaken for them. The immediate cause of the formation of these cysts seems to be in a contraction taking place in some part of the course of the tubes, either from an increase in the connective tissue around the gland, or from some change in the *membrana limitans*. The part below the contraction becomes dilated with the products of secretion, while the gland-tube above is atrophied. Sometimes two cysts may be formed in the same gland. At a later period the epithelial contents may undergo atrophy, and the cysts remain filled with a tenacious colloid matter. These cysts have been found associated with similar formations in the rectum and upper part of the digestive tube, especially in the uvula, and may possibly correspond with the condition described by Professor Simpson, under the title of 'Chronic Pellicular, or Eruptive Inflammation of the Intestinal Mucous Membrane.' The translucent appearance observed by the naked eye in patches of the mucous membrane, is found to be caused by fatty degeneration of the epithelium of groups of glands. It is not unfrequently found around spots of hæmorrhagic erosion. To the same cause are due whitish spots seen deep in the substance; but

the difference of appearance in the two cases does not seem easily explicable. Opaque, white spots, apparently superficial, are found to be due to a fatty degeneration of all the elements of the membrane, and especially of the cells of the connective tissue. They may, by breaking down, give rise to a loss of substance at the parts, and the process of their formation and disintegration offers a striking analogy to the fatty erosion of the arterial coats as described by Professor Virchow.

There is a considerable analogy between the changes produced by these inflammatory affections in the stomach and those which are met with in the kidney, tending to illustrate the laws of diseases of glandular organs in general; and though the difference in the structure of the organs causes considerable variation in the forms produced, yet the essential characters are very similar in both. In the acute stage they are chiefly evidences of increased nutritive activity, called forth by the inflammatory stimulus as shown by the enlargement of the epithelium and the increase of protein contents in the interior of the cells. In the more chronic forms the connective tissues are those principally affected by a tendency to hypertrophy; while the gland-cells undergo a fatty degeneration, or atrophy; and the parallel is complete, even to the formation of cysts caused by similar changes in the gland-tubes of both organs.

The causes of catarrhal conditions of the stomach are, as pointed out by other observers, to be found with greatest frequency in diseases obstructing the general circulation; and changes belonging to the chronic type are found with a paramount frequency in phthisical patients, though no case of tubercle of the stomach has been observed by the author. Acute catarrhal affections appear to be not unfrequently associated with septic or other acute general diseases, and have been observed in cases of puerperal fevers and cholera, where the kidneys presented, both to the naked eye and under the microscope, the appearances observed in the first stage of Bright's disease. Chronic induration of the kidney, associated with fatty degeneration of the renal epithelium, and increase of the connective tissue, has also been observed coincidently with similar affections of the stomach.

ART. 65.—*On the changes taking place in Cirrhosis.*
By Dr. BEALE, Physician to King's College Hospital.

(*Beale's Archiv. of Med.*, No. ii, 1858.)

"From the different points which have been demonstrated," says Dr. Beale, "one is led to conclude that in cirrhosis the change always commences in the cells, near the portal aspect of the lobule, and gradually progresses towards the centre. The cells at the circumference of the lobule being exposed to the action of blood overcharged with deleterious substances (alcohol, or substances resulting from its decomposition) recently absorbed from the intestine, deteriorate in structure. In consequence of the altered state of the cells, they cease to exert that attractive force, which they possess in health in an eminent degree, upon the portal blood as it flows through the capillaries. The blood, therefore,

circulates more slowly, and tends to accumulate in the organ. The branches of the portal vein become unduly distended.

"This state of things may only be of temporary duration. The cells being again supplied with healthy blood may resume their normal functions; or, the causes, which first gave rise to the derangement, still continuing, will produce further and permanent alterations.

"The congestion caused at first would gradually pass off, as the blood found its way back to the cava by other channels. The capillaries of the lobule no longer called upon to transmit the normal amount of blood, would deteriorate in structure, and their capacity would diminish. The small amount of blood distributed to the lobule under these altered conditions, would reach the intralobular vein by the most direct route, and the communication would be kept up by one or two straight branches passing directly through the lobule, from the branches of the portal, to those of the hepatic vein.

"The wasting of the cells and shrinking of the vessels would still proceed, and the whole organ would diminish in bulk.

"The trunks of the portal vein no longer required to transmit the enormous amount of blood which they do in health, would shrink, and the other large vessels would suffer a corresponding change.

"At the same time that the lobules were diminishing in size, the smaller interlobular fissures, in consequence of the shrinking of the vessels they contained, would scarcely be visible, and hence several lobules would appear to be fused together, separated here and there from other collections by intervals corresponding to portal canals, containing only vessels and ducts, but increased in diameter, by the shrinking and alteration of the lobules which formed their boundaries.

"The impeded circulation through the liver would seriously interfere with the onward course of the blood from the intestines, and a condition of the intestinal capillaries very unfavorable for absorption of the constituents of the food would be induced, and this would be aggravated by the presence of bile impaired in quality and insufficient in quantity. At length the action of the alimentary canal would become more and more disturbed, the blood deteriorated, and consequently the actions of other organs more or less interfered with. The impediment to the circulation through the liver, and the state of the blood, favour the transudation of serum into the peritoneal cavity. The digestive process becoming much deranged, the whole organism would suffer in nutrition, all the nutritive functions are impaired, and the powers of the patient at length exhausted.

"Such is a rough history of the changes occurring in cirrhosis, as deduced from an examination of the liver after death, and a consideration of the symptoms which manifest themselves in the course of the disease. Although these views differ materially from those generally entertained with regard to the nature of the affection, they are the only ones which I believe will be found in accordance with the morbid changes which have been demonstrated.

"Let it be supposed for a moment, that lymph were actually effused into the portal canals as described, which lymph by its contraction and subsequent conversion into fibrous tissue impeded the flow of blood to

the lobules, how are we to account for the numerous large and small vessels which still remain pervious in the portal canals? And why should the capillaries in the lobule for the most part cease to transmit blood, while those in the portal canals allowed fluid to pass through them readily? The contraction of such lymph would certainly compress the ducts, and cause them to waste; but I have shown that these are demonstrable, both in injected and uninjected preparations. The so-called fibrous tissue is traversed in every part both by vessels and ducts, and in many situations the coats of these channels are in such close contact as to render the existence of this fibrous tissue absolutely impossible; and it is doubtful if the fibrous appearance certainly observed in some situations in uninjected specimens, is not due rather to the remains of wasted and shrunken vessels and ducts than to the presence of an adventitious tissue.

“The *very gradual* alteration and wasting of the cells, with progressive shrinking of the lobules from the circumference towards the centre, and the degeneration in the capillaries of the lobule, evidently resulting from impaired nutrition and inaction, are not easily explained by the view, that cirrhosis depends upon impediment to the circulation in the portal vessels, caused by the contraction of inflammatory products effused into the portal canals. The persistence of pervious vessels in considerable number in the portal canals and interlobular fissures, and the existence of ducts which may be readily injected, are quite incompatible with such an explanation.

“From a careful examination of the parts, then, one is led to conclude that the morbid changes in cirrhosis are not dependent upon inflammation, neither is there any evidence whatever of the presence of any tissue which by its contraction would lead to the alterations in the structure of the gland which have been demonstrated.

“The first morbid change in cirrhosis affects the cells, and the subsequent alterations result from this, according to well-known physiological laws.

“Such a view of the pathology of cirrhosis naturally suggests observations on the treatment of the disease, but these I must postpone until many other cases have been the subjects of observation.”

ART. 66.—*Aneurism of the Hepatic Artery opening into the Gall-bladder.*
By Dr. LEBERT.

(*Archiv. f. path. Anat. u. Physiol.*, T. xiii, 1858.)

CASE.—A woman, æt. 30, who suffered for some weeks from a sense of weight in the epigastrium. On the 28th May, 1855, she felt a violent pain in the neighbourhood of the stomach, and vomited a large quantity of blood, which vomiting was repeated on several occasions on the following days, until she became anæmiated to the last degree. Epileptiform convulsions, repeated more than once, and symptoms of slight jaundice preceded death, which happened from exhaustion on the 6th of July following. On examination afterwards, the gall-bladder, the biliary duct, and the ductus choledochus were seen to be filled with coagulated blood. It was seen, also, that the gall-bladder communicated by a small opening with an aneurism in the hepatic artery, and that the bladder and this aneurism were intimately united. The

aneurism itself was seated anteriorly to the vena porta, and the size was that of a pigeon's egg. Within it were several concentric layers of fibrine. The mucous membrane of the stomach was healthy, and no other lesion could be detected elsewhere.

ART. 67.—*On the symptoms caused by the presence of Round Worms in the Biliary Canals.* By Dr. E. A. BONFILS.

(*Archiv. Gén. de Méd.*, June, 1858.)

M. Cruvelhier denies positively that round worms can penetrate into the biliary canals during life. M. Bonfils, on the other hand, relates twenty-three cases, collected from various sources, which show that this opinion is incorrect. The cases also show that we may, to some extent, hope to diagnose such an accident during life. The symptoms are, violent pain in the region of the liver, accompanied by yellowness of the skin, vomiting, &c., coming on intensely and suddenly, without any other assignable moral or physical cause—the intenseness and suddenness being similar to that of the symptoms belonging to the presence of a biliary calculus in the same passages,—and followed by the rejection of the worm, and the rapid subsidence of all the symptoms.

ART. 68.—*The symptoms of Cancer of the Pancreas.*
By Dr. DA COSTA.

(*North American Medico-Chirurgical Review*, Sept., 1858.)

The following passage is taken from a paper based upon thirty-seven cases, which are carefully tabulated by the author. As we might expect, the symptoms of this disease are mostly produced by the effects of the disease upon the organs—few, if any, being special, none constant.

“The main symptoms,” says Dr. Da Costa, “of pancreatic cancer are a tumour in the epigastric region, pain there, or in the back, constipation, progressive emaciation and debility, and obstinate jaundice and occasional vomiting, as the disease advances. The diagnosis is possible, if these symptoms be present, and provided we are able to exclude with certainty the diseases of the stomach and of the liver. I shall not attempt to decide in how far the symptoms may be shared by other chronic affections of the pancreas. Tubercle of that organ is rare, and is associated with tubercle of the lung or of the brain. Chronic pancreatitis gives rise to many of the same phenomena; but, taking the cases which I have met with in pursuing this inquiry as my standard, I should say that those signs which indicate a tumour, and the symptoms which show its marked growth and pressure upon other organs, are not often present; that pain does not occur to such a marked degree; that the falling off in health is very gradual, and the disease slower of progress, and also that the bowels are not as constipated, but are, on the contrary, more frequently relaxed. It is, however, fair to state, that Dr. Claessen, in a work on ‘Diseases of the Pancreas’ (Cologne, 1842), remarks that constipation in chronic pancreatitis is urgent and enduring.”

ART. 69.—*On the treatment of Dysentery by large doses of Ipecacuanha.*
By Mr. DOCKER, Surgeon of the 2d Battalion of the 7th Royal Fusiliers.

(*Lancet*, July 31 and Aug. 14, 1858.)

"As surgeon of the 5th Fusiliers," says Mr. Docker, "I was stationed in the Mauritius nearly six years, and had, therefore, ample opportunity of becoming acquainted with this hitherto intractable and fatal disease. I say "hitherto," as it is my firm belief that, henceforward, dysentery may be as much under control and as expeditiously cured as simple diarrhœa.

"For the greater part of the above period—viz., from 1851 to 1857—I had availed myself of the remedies in general use. At last, disheartened with my ill success in several bad cases, wherein I had perseveringly but ineffectually employed the *secundem artem* treatment, and remembering to have somewhere seen it mentioned that the powdered root of ipecacuan, in large doses, had been given with great effect in this complaint, I became anxious to make trial of an agent declared to be of such extraordinary efficacy.

"I have tested this medicine in cases of every kind and degree. Out of upwards of fifty cases of dysentery I lost but one (in former years the mortality ranged from 10 to 18 per cent.); and in the instance in question death was caused by abscess in the liver: the primary disease had been not only cured, but very thoroughly cured, as I shall hereafter show. I must observe that I had at one time been in the habit of prescribing ipecacuan in the small doses recommended by Mr. Twining; but so ineffective was it when thus administered,—excepting in cases of no great severity, wherein other medicines answered as well, *without* the inconvenience of nauseating,—that I had long ceased to employ it. On resuming the use of ipecacuan, I gave it in doses ranging from ten to ninety grains; rarely less than twenty grains. The larger quantity was given in urgent cases only, the ordinary dose being a scruple or half a drachm. The action of these large doses is certain, speedy, and complete; and truly surprising are sometimes their effects. In no single instance has failure attended this medicine, thus employed. I am not, of course, sufficiently sanguine to expect that it will invariably succeed; but of this I am convinced, that it will effect a complete cure in an immense majority of instances.

"In all constitutions, robust as well as delicate, under all circumstances, the result is the same. In the very worst cases, when the strength of the patient is almost exhausted, after the whole range of remedies has been tried in vain, the disease running its course swiftly and surely to a fatal issue, ninety grains of ipecacuan have been given, and forthwith the character of the disease, or, I should rather say, the character of the *symptoms* has been entirely changed; for the disease itself is literally cured, put a summary stop to, driven out. The evacuations from being of the worst kind seen in dysentery, have, not gradually, not by any degrees, however rapid, changed for the better; they have ceased at once, completely. There has been no inclination even to stool for twenty-four or thirty-six hours, the patient all the time in a state of delightful

ease and freedom from pain; then at last, without aid of any kind, a perfectly natural, healthy evacuation, all irritation, pain, and tenesmus, having at the same time entirely ceased.

“Nor is there the disposition to relapse so common in acute dysentery. I have not observed what may be termed a true relapse in any instance. If the patient contracts dysentery again, he does so *de novo*. All that remains—the medicine having cut short the disease—is for the patient to recover strength; and this quickly follows, without any extraordinary care as regards diet and regimen, so indispensable and requiring such nicety of management in convalescence from dysentery generally. The usual necessity, moreover, for after treatment, in the shape of a long course of astringents, &c., is in most cases entirely obviated, a few doses of some vegetable tonic being all that is needed.

“It may be asked by what means the stomach is enabled to retain such large doses of an emetic substance. The course I have generally adopted is as follows: In the first place, a sinapism is applied over the region of the stomach, and simultaneously a draught given containing a drachm of laudanum. Half an hour after, when the sensibility of the stomach has been, by the action of the opium and counter-irritant, as much as possible diminished, and the patient’s attention is occupied with the sinapism or by conversation, the ipecacuan is administered—generally in a draught, sometimes in the form of pill or bolus—and the semi-recumbent posture steadily maintained. In a considerable proportion of cases the medicine is not rejected, or it is at least retained long enough to enable it to do its work. If necessary, I repeat it till the stomach does retain it. I never yet have been obliged to give it in the form of enema. Where so considerable a dose as sixty or ninety grains has been administered, I in general wait ten or twelve hours before giving another. Should the bowels, however, not meanwhile have acted, a repetition is not generally required. I ought here to mention that I begin the treatment of dysentery, in most cases, with an emetic—always with a thorough clearance of the bowels.

“To those acquainted with tropical dysentery, the facts I have stated relative to the action of large doses of ipecacuan may appear almost incredible; the following cases, however, all of which were under my own immediate care, will, I trust, prove that I have not exaggerated:”

CASE 1.—Private J. H.—, æt. 26; admitted April 1, 1855. This man’s symptoms were decidedly dysenteric, (I do not transcribe the case verbatim, as it would occupy too much space,) “stools scanty, containing blood and mucus, accompanied with severe tenesmus, and tenderness on pressure over the descending colon.” An emetic and purge were given at the outset, then turpentine in ten-minim doses, with a grain of opium every four hours. This answered very well at first, for on April 2d the report was “stools entirely feculent, semi-fluid, homogeneous, and of dark colour.” And the motions *continued* feculent, though action of the bowels was frequent.

On the morning of the 7th, (small doses of turpentine, with laudanum and astringents, had been continued up to that time,) the report was “seven or eight natural semi-consistent stools during the last twenty-four hours.” Ordered, powdered calumba, one scruple three times a day. This, however, proved to have been premature, for on the evening of the same day, an unfavourable change had taken place. “Bowels moved five times since morn-

ing; small quantities of feculent matter, with much blood and mucus." Ordered, sinapism to the epigastrium, and three grains of opium; half an hour after, ninety grains of ipecacuan in the form of draught. On the following morning the report was, "Bowels moved three times, very copiously, during the night; stools watery and feculent, and containing *no trace of dysenteric matters*. He retained the ipecacuan four hours, and then vomited. Is quite free from pain." There was no occasion to repeat the ipecacuan, for not a drop of blood or mucus was afterwards seen, and he was discharged, completely cured, on the tenth day from admission.

CASE 2.—Private A. C—, æt. 19, an exceedingly delicate, weakly lad, admitted on the 26th of December, 1855, "with frequent purging of scanty stools, consisting of a little feculent matter, mingled with sanious mucous; tenesmus severe. Ill two days prior to admission." In this case, ipecacuan was employed at the outset; scruple doses with twenty drops of laudanum in a draught every four hours. Sinapism to the entire abdomen.

27th.—Action of bowels very frequent since admission—upwards of twenty times; stools of natural appearance, but copious and watery; tenesmus less severe. (It ought to be mentioned that, arriving from England with a batch of recruits in the month of September previously, he had, since landing in the island, scarcely ever been free from diarrhœa.) Draughts and sinapisms repeated.

28th.—Bowels moved eight times yesterday—evacuations less watery, and five times in the night, when the stools were semi-consistent; no blood nor mucus. Ordered, compound soap pill, five grains, every six hours.

29th.—One very scanty semifluid stool only since last report. Infusion of gentian three times a day.

30th.—No motion since yesterday. Gentian continued.

On the 31st, the bowels being still confined, they were gently moved with castor oil.

The patient was discharged quite well on the 9th of January. Eight days after, it was necessary to re-admit him on account of diarrhœa. Ordered, mercury with chalk, quinine, and Dover's powder every four hours. Next day he was better. On the 16th, however, there was a trace of blood in the stools. Ten grains of ipecacuan were added to each powder (every four hours).

Jan 17th.—The blood had disappeared; stools were semi-consistent. He went on very well, gradually gaining strength, till the 24th, when diarrhœa returned.

25th.—Stools now contain blood and mucus, and are attended with straining. Ipecacuan renewed in ten-grain doses, every four hours.

26th.—Bowels not moved once since yesterday; three times during the night; stools semi-consistent, feculent, and intimately mingled with tenacious mucus. Ipecacuan draughts continued.

27th.—Stools of much better appearance.

On the 28th, they were "perfectly natural," and so continued, with occasional relaxation, but free from the least trace of dysenteric matters for eleven days; then, on the evening of the 8th of February, the report was, "Bowels moved twenty times since morning; evacuations scanty, and consisting wholly of sanious mucus." Ordered sinapism over the stomach, and draught containing twenty minims of laudanum; half an hour afterwards, sixty grains of ipecacuan.

Feb. 9th.—Up very little during the night, passing, altogether, not quite half a tea-cupful of sanious mucus; tenesmus, but no pain in the abdomen. Ordered, castor oil, twenty minims; mucilage, one ounce; ipecacuan powder,

one scruple; tincture of opium, ten minims; peppermint water, one ounce, every four hours.

On the 10th, the only change observable was, that very little blood was passed. Ipecacuan powder, ten grains; tincture of opium, twenty minims; camphor mixture, one ounce; liquor acetate of ammonia, half an ounce, to be taken every four hours.

Under this treatment he daily improved, and on the 13th the stools were "few and perfectly natural."

After a second complete intermission of fourteen days, during which he was only kept in hospital for the recovery of his strength, he *again* had a return of dysenteric symptoms, "seventeen or eighteen stools, feculent at first, but latterly tinged with blood; tenesmus, with tenderness on pressure over the abdomen generally." Once more recourse was had to the ipecacuan draughts, as on the 10th, which had answered so well.—Evening: A few drops only of sanious mucus passed since morning. Ordered, castor oil, two drachms.

28th.—Purged seventeen or eighteen times during the night, and has passed a quantity of healthy feculent matter. Draughts repeated; also on the 29th of March.

April 1st.—The report was "stools perfectly natural," and his bowels continued composed till the 7th, when the stools again contained a little mucus and blood. Ordered the following draught, every six hours: Oil of turpentine, ten minims; mucilage, half an ounce; tincture of opium, twenty minims; powdered ipecacuanha, ten grains; peppermint water, one ounce.

On the 8th and 9th (the draughts being continued) the action of the bowels was frequent.

10th.—No change having been made in the treatment, the motions were "natural and *formed*."

After this he had no return whatever of dysenteric symptoms, but was so excessively weak that I could not safely discharge him before the 29th of April. He has since continued well, and perfectly free from his besetting ailment—diarrhœa.

The above case is a very good exemplar of the powers of ipecacuan in dysentery. Without so powerfully controlling an agent, I look upon it as morally certain that this boy would have died; for never, in the whole course of my service, had I to deal with a case in which the disposition to morbid action in the bowels was so marked. It will be observed that though there were returns of dysenteric symptoms, *yet during the intervals their cessation was complete*. I think that chills or checked perspiration—conditions it is impossible entirely to guard against in the Mauritius, especially at night and in the early morning—were the causes of the repeated attacks in this case.

CASE 3.—Private S. M.—, æt. 25; admitted with dysentery on the 11th of February, 1856. The symptoms were at first slight, and the treatment simple, (chiefly purgatives and Dover's powder,) and he went on very well till the 17th, when the report was, "Four stools since yesterday, scanty, and consisting entirely of blood and mucus." Ordered powdered ipecacuanha, one scruple; Dover's powder, half a scruple: to be taken every four hours.

February 18th.—"Four stools since last report, feculent and formed, with a trace only of sanious mucus; patient quite free from pain and tenesmus." Powders continued, with the addition of one grain of opium to each.

19th.—"Stools entirely feculent and consistent." He was discharged fit for duty on the 1st of March, having had not the slightest return of dysenteric symptoms after the 19th of February.

CASE 4.—Private W. B—, æt. 36; a very weakly phthisical subject, with a marked disposition to atonic diarrhœa. Admitted with dysenteric symptoms on the 18th of March, 1856. "Purging frequent, with severe tenesmus; stools watery, and contain both blood and mucus." He was ordered an emetic immediately, followed by an ounce of castor oil, and a grain of opium. Evening: "Purged nine times since admission; evacuations copious and watery, with a large admixture of fluid blood." Ordered forthwith the sinapism and drachm of laudanum, and half an hour after, sixty grains of powdered ipecacuan in form of pill.

19th.—"Feels much better: bowels moved seven times during the night; stools liquid, feculent, and containing very little blood and mucus; tenesmus considerably diminished." "The ipecacuanha was retained." Ordered a draught, every four hours, composed as follows: Oil of turpentine, twenty minims; mucilage, half an ounce; peppermint-water, one ounce; powdered turpentine, one scruple; tincture of opium, twenty minims.

20th.—"Four stools yesterday of better appearance, two during the night; a few drops of pus-like (the most harmless) mucus only perceptible; feels much better; tenesmus entirely gone." Draughts of ipecacuan and turpentine continued, with the addition of twenty drops of castor oil to each.

21st.—"No motion yesterday; three during the night, natural and semi-consistent." Draughts discontinued.

22d.—"Two perfectly natural stools since last report." Ordered a scruple of powder of calumba, three times a day.

23d.—"Improvement maintained;" and he was discharged quite well on the 28th.

There could not well be a more satisfactory case than the above. A debilitated, delicate subject, attacked with dysentery—and while the attack lasted it was *severe*—is cured in ten days, and so *radically* cured as to have had no return whatever of bowel complaint, though previously much disposed thereto.

This case exhibits the specific action of large doses in dysentery in a striking manner. At that time I was not fully conversant with this medicine, and as the man was extremely ill at the time of admission I deemed it advisable at first to employ calomel; and this medicine, which by many is looked upon as specific in dysentery, had a fair trial—so fair, indeed, as to place the life of the patient in considerable jeopardy. At this juncture it will be observed by those practically conversant with dysentery that the man's symptoms were indicative of extremest danger. Fortunately, recourse was had to ipecacuan; and this medicine was given in full (drachm and a half) doses three times. But I must not anticipate. As this case is so interesting, I make no apology for transcribing it nearly in full:

CASE 5.—Private J. T—, æt. 28, admitted March 18th, 1855; a slight, narrow-chested, delicate man. Has frequent purging of copious stools, consisting mostly of fluid-feculent matter, with some admixture of mucus and much blood. Says "he has no pain in the belly, no tenderness on pressure." This was taken *cum grana salis*, for there was an evident disposition to make as light as possible of his complaint: he knew he had been guilty of disobedience of orders in not coming to hospital sooner. Closely questioned, he at last confessed that he had been ill for several days before reporting himself sick.) Admits having tenesmus. He was under treatment for acute dysentery in April, 1852. Ordered an emetic immediately, and every four hours a draught consisting of oil of turpentine, ten minims; acacia mucilage, half an ounce; tincture of opium, twenty minims; peppermint-water, one ounce.

In the evening the report was : "Has passed since morning two scanty dysenteric stools." Ordered half an ounce of castor oil and twenty drops of laudanum.

March 19th.—Eight motions during the night, copious, feculent, and semi-fluid, with some froth tinged with blood on the surface; pulse 92, soft; tongue coated in the centre. To continue draughts of turpentine, &c.—Evening: Bowels have acted three times since morning; stools scanty, and of a highly dysenteric appearance. Ordered ten grains of calomel and one of opium immediately; the same to be repeated at four o'clock in the morning.

20th.—Has had during the night eight or nine motions, fluid. Dark-coloured, feculent, with a little mucus, and more blood on the top; straining very severe, and there is much tenderness on pressure over the cæcum; pulse 132; soft and rather full; tongue furred. Ordered—calomel, two grains and a half; tartar emetic, one-eighth of a grain; hydrochlorate of morphia, one-sixth of a grain, every four hours.—Evening: Bowels moved eleven times since morning; stools more dysenteric in appearance, with less of feculent matter. A sinapism was ordered to be applied immediately over the stomach; internally, sixty minims of laudanum, and half an hour after, a draught consisting of a drachm and a half of ipecacuan to an ounce and a half of water.—Ten p.m.: The report was that he retained the ipecacuan draught two or three minutes only; has been moved twice since six p.m.; stools very bad indeed, quite liquid, with hardly a trace of feculence, consisting chiefly of a little mucus and a very large proportion of fluid blood; he is excessively weak; pulse rapid and thready, intermittent; surface cold, and bathed in perspiration; tenesmus severe. The opiate draught was now repeated, but this time with twenty minims only of laudanum; half an hour after, ninety grains of ipecacuan, as at six o'clock.

21st.—Has passed a tolerable night, and feels better, bowels not having been once moved since the administration of the last dose of ipecacuan, which he retained an hour and a half, then vomiting three times. He feels nausea at present. Is perfectly composed, and free from pain or irritability. Pulse 120, full and soft; tongue furred, but moist.—Evening: No action of the bowels since morning; the draught was retained three hours: he then vomited once. To have, at bed-time, a draught composed of liquor acetate of ammonia, half an ounce; liquor acetate of morphia, thirty minims; tincture of matico, and compound tincture of lavender, of each one drachm; peppermint water, one ounce.

22d.—Marked improvement in every respect. After an interval of thirty-four hours his bowels have at last acted, during the night, once only; stool scanty, semi-consistent, feculent, and homogeneous, without a trace of blood or mucus. He is entirely free from pain or tenesmus, and perfectly comfortable in every way. Draught last ordered to be continued every six hours.

23d.—Improvement continues. No motion since last report. Pulse 100, jerking; tongue coated. Ordered, camphor mixture, one ounce; liquor acetate of ammonia, half an ounce; disulphate of cinchona, two grains; tincture of lavender, one drachm: to be taken every four hours. Chicken broth; brandy, half a gill.

24th.—One scanty, consistent, entirely feculent stool; pulse 90, soft; tongue cleaning. Ordered, infusion of gentian, two ounces; disulphate of cinchona, two grains; three times a day. Brandy, one gill.

25th.—Improvement maintained. No motion. Gentian draughts repeated.

26th.—Continues to get better; one natural evacuation. Tonic draughts continued. Broiled chicken.

27th.—Same report. He is very weak. Draughts continued. Ordinary diet, and brandy.

29th.—Convalescent. Gentian and cinchona draughts continued.

April 1st.—Same report. Draughts continued.

3d.—He is still rather weak.

4th.—A trace of mucus in the stools (three since last report), and there is slight tenesmus. Ordered, castor oil, two drachms; gentian and cinchona draughts continued.

5th.—No motion since last report, nor has he any inclination to stool. Ordered, castor oil, half an ounce; tonic draughts continued.

6th.—Bowels moved three times, after last dose of oil; stools natural. Draughts continued.

7th.—Some thick, yellow mucus only passed since last report. Castor oil, two drachms, immediately; a scruple of powder of calumba three times a day.

8th.—Has passed three feculent stools, entirely free from mucus, since taking the oil. Calumba continued.

9th.—No motion since last report; has nearly recovered his strength. Castor oil, one drachm; calumba powders continued.

11th.—Discharged cured.

A more remarkable case than the above could hardly be. It is an unquestionable fact that this man's life was saved by ipecacuan, given in the doses it was, *and by ipecacuan alone*, for the opium *only* aids in enabling the ipecacuan to be retained. Moreover, I am convinced that, in the condition he was on the evening of the 20th March, by no other known means could he thus, as it were, have been snatched from the brink of the grave. Instead of dying, however, this soldier was at his duty, completely cured, in little more than three weeks from his admission, desperately ill, into hospital. It will, I think, be conceded, that this last case, if not those preceding it, ought to secure for the ipecacuan in large doses treatment at least a fair trial.

“Another and more certain benefit resulting from this treatment is the apparently entire obviation of chronic dysentery, with its many and protracted miseries. Who that has had to contend with a wearisome and disheartening case of this kind will not hail with delight a remedy which enables him to effect a cure in the same number of *days* that formerly would have occupied *weeks* or *months*; a cure, moreover, so *complete* as to send his patient with a new lease of life, actually better after his illness than he was before!

“As regards the *rationale* of the action of ipecacuan in large doses, I will not venture on so debatable a point to express an opinion. That it is a very energetic *tonic* is sufficiently evident; equally certain that it is a most powerful *styptic* (this being the effect of its tonic property), and as such likely to be of great use in some active and in most passive hæmorrhages, especially in those occasioned by exudation from mucous surfaces. In dysentery, at all events, the value of this medicine is incontestible. I believe the time may come when it will be considered as much a specific in this disease as bark is in ague, and sulphur in itch.”

ART. 70.—*On the Differential Diagnosis of Ascites and Ovarian Dropsy.*
By Dr. ———.

Medical Times and Gazette, June 5, 1858.)

There is one sign which hitherto we have never known to fail, but which is, we believe, as compared with its value, but little known. This sign is obtained by percussion of the lumbo-lateral region. If in a case of ascites, in which the distension is so great that the hydrostatic line of level in front is not changed by posture—and it must be remembered that only in ovarian cases, in which the cyst is so large as to simulate this extreme condition, ought any difficulty to occur—if, in such a case, the patient be made to sit up in bed, and the loins be percussed, it will be found that the note is the same (usually dull) on both sides. If an ovarian case, no matter how great the distension, be treated in the same way, one loin will be found to be clear, and the other quite dull. The explanation is obvious: in ascites, the air-containing coils of the gut float as far forward as their mesenteric attachment will permit, while in the case of an ovarian cyst, they are pushed over to the healthy side. It is not easy to conceive any condition of things, excepting entire exclusion of air from the whole tract of intestines, which could diminish the trustworthiness of this symptom. It indicates, also, with unfailing accuracy, on which side the ovarian cyst, if it exist, has originated.

ART. 71.—*On the use of Kamala (Rottlera Tinctoria) as a Vermifuge.*
By DR. RAMSGILL, Physician to the Metropolitan Free Hospital.

Kamala has little smell or taste; it is insoluble in cold and nearly so in boiling water; but yields an admirable and effective tincture. It is this tincture which Dr. Ramsgill uses, as prepared by Messrs. Hanbury, of Plough-court.

According to the author's experience thus far, the alcoholic preparation of kamala is more uniformly effective than oil of male-fern or than spirit of turpentine, and infinitely less disagreeable than either. In no case where a tænia was known to be present in the intestinal canal, evidenced by fragments occasionally passing away, has it failed to effect its expulsion; and in most cases where the head of the worm has been carefully looked for, it has been found.

CASE 1.—On March 3d, John F—, æt. 22, residing in St. George's-in-the-East, was admitted a patient of the Metropolitan Free Hospital, with tape-worm. He first observed that he passed fragments of them six years since. Has passed as much as eleven yards at once; very rarely passed a week without having seen fragments. Formerly was at sea, and lived for long periods on salt meat, both pork and beef; likes salt and vegetables. Has taken turpentine, which did good for a few weeks; objects to take it again. Ordered, tincture of kamala, two drachms; distilled water, four ounces. To be taken night and morning. To have three doses.

March 6th.—By some mistake, the patient swallowed the tincture without water. Felt no result from the first or second dose, but vomited the third,

at the same time three or four series of links some inches long passed away, including one tapering to a point (this he had been asked to look for). To repeat the three doses in water.

10th.—The first dose caused purging; the second, violent diarrhœa and vomiting for forty-eight hours. No worm nor fragments passed. Has now eczema of the scrotum, which he insists has been caused by the medicine.

CASE 2.—Morris H—, æt. 63, was admitted a patient of the hospital on March 6th. He lives in Glasshouse-fields, Ratcliffe-highway; has had tapeworm five or six years, and has passed long pieces after taking turpentine. This drug has often cured him for six months, but never for a longer period. He is not fond of salt nor of vegetables; eats pork occasionally; suffers considerably from epigastric nervous sensations, faints, &c. Ordered tincture of kamala, two drachms and a half; water, one ounce: to be taken night and morning. After the second dose some "thousands of bits came away;" the third and last dose purged him very much, and made him feel sick, but he did not vomit. No head found, probably on account of carelessness in the search for it. The bowels continued irritable for some days, but no more fragments of *tænia* appeared.

CASE 3.—Mary P—, æt. 21, admitted March 6th. Had tapeworm two years; is very feeble; appetite voracious or absent; complains greatly of epigastric fainting; dislikes salt, scarcely ever eats it with food; always fond of pork, eats it frequently. Ordered tincture of kamala, two drachms; water, four ounces: to be taken night and morning. The physic operated six times; no pain, no vomiting; worm passed within twelve hours after taking the first dose; head found.

On the 17th she had not passed any further fragments, and observed that she felt better in health than she had done for years.

CASE 4.—Emma H—, æt. 24, resides at Stepney; admitted a patient of the hospital on March 6th. She first saw fragments of *tænia* in the evacuations three years ago, and has seen them occasionally ever since. Suffers frequently from fits of "retching, lasting four or five hours together," and is not by any means able to stop it. She attributes this to the presence of *tænia*. Has suffered from it about three years—*i. e.*, since about the time she first discovered fragments of worm; likes salt, and often eats pork. Ordered tincture of kamala, two drachms; water, four ounces: to be taken night and morning for three times.

March 10th.—After taking the second dose a great number of fragments passed; none after the third; did not find the head, perhaps did not examine sufficiently closely. She had no pain, purging, nor vomiting, but felt sick, and suffered much for the two days when taking the medicine with headache. To have three three-drachm doses of the tincture as before.

17th.—The physic has made her violently sick, but it was not returned; no diarrhœa. She has not seen any more segments of the worm.

This patient has for the present disappeared, so that it cannot be determined whether the retching was caused by the *tænia*.

CASE 5.—A. L—, æt. 36, resides in Whitechapel. First saw that he passed portions of tapeworm three years ago; has suffered during this time with all sorts of nervous feelings; is "always in a twitter;" he is a member of the Jewish Church; they do not eat pork, he dislikes salt, and takes little vegetable with his dinner. Has taken turpentine three times; this drug punished him severely; he could not for a time pass urine. Has also taken the oil of male fern, after which he vomited greatly; he was much purged, and became delirious for seven or eight hours. He was ill for three weeks afterwards, in bed, and under treatment.

March 20th.—Tincture of kamala, two drachms; water, two ounces, night and morning (three doses).

23d.—Saw three or four joints after the second dose; no vomiting nor diarrhoea. On examination I find he has by mistake only taken one-drachm-and-a-half doses. To increase each draught to three drachms night and morning.

26th.—The first dose caused vomiting, but the medicine was not returned. After the lapse of half-an-hour, twelve yards of the worm, including the head, was passed. The second dose caused vomiting and diarrhoea, as did also the third, no further segments of worm passing.

(E) CONCERNING THE GENITO-URINARY SYSTEM.

ART. 72.—*On Amblyopia and Amaurosis in their relation with Bright's Disease.* By Dr. A. WAGNER.

(*Virchow's Archiv.*, T. xii, liv. 2 and 3, 1858.)

The connexion of the amaurosis which may accompany Bright's disease with certain definite lesions of the retina has been pointed out by M. Türk. In 157 cases of this disease, during a period of four years, M. Wagner has met with eighteen cases in which this connexion was very evident when recourse was had to the help of the ophthalmoscope. These cases are given in detail.

ART. 73.—*Alteration of vision in Bright's Disease.*
By Dr. LECORCHÉ.

(*Archiv. Gén. de Méd.*, Aug. 1858.)

M. Lecorché has devoted his inaugural thesis to the elucidation of this subject, and his principal conclusions are these:

1. Alterations of vision (amblyopia, amaurosis) are of frequent but not constant occurrence in Bright's disease. In seventeen cases, the author met with them in seven instances.
2. Youth, the female sex, pregnancy, or recent childbirth, and the chronic form of the kidney-disease, predispose to the development of amblyopia.
3. There is no direct relation between this degree of alteration in the vision and the extent of kidney-disease, or the amount of albumen in the urine.
4. The existence of amblyopia is not of necessity a warrant for a dismal prognosis, nor is it necessarily a permanent condition.
5. In some cases there are no evident organic lesions in the eye; in others, these lesions may be rendered very evident by the ophthalmoscope or by examination after death. These lesions are passive or active congestion of the retina, or degeneration of a fatty character.

M. Lecorché relates three original cases, and collects others from the writings of MM. Landouzy, Forget, Avard, Dewarres, Wagner, and others.

ART. 74.—*The temperature of persons suffering from Diabetes.*

By Dr. LOMNITZ.

(Henle and Pfeuffer's *Zeitschrift*, 3 Reihe, Bd. ii, Ht. 3, 1858.)

These observations were made upon three patients, one a young man æt. 19, and two women aged respectively 30 and 36. They extended over twenty-one days consecutively. Every morning and evening M. Lomnitz took the temperature of the mouth and armpit, and noted at the same time the frequency of the pulse and respiration, and the quantity and density of the urine passed during the previous twenty-four hours. On comparing the mean temperature with the normal temperature as determined by M. Bärensprung, he found that in diabetes the temperature of the armpit had fallen $1\cdot07^{\circ}$ Reamur, and that of the mouth $0\cdot39^{\circ}$. He found, also, that this depression of temperature had no evident connexion with the variations of the pulse.

ART. 75.—*On the relation between Diabetes mellitus and Carbuncle.*

By Dr. A. WAGNER, Physician to the Dantzic Hospital.

(Archiv. für Path. Anat., Bd. iii, Heft. 4 and 5; and *Med.-Chir. Review*, July, 1858.)

The coincidence of inflammatory and gangrenous affections of the skin with melituria has been observed by Prout, Marchal de Calvi, Landouzy, Dr. Goolden, and others. Dr. Wagner inquires into the relation the two affections bear to one another, and arrives at the following conclusions, which he supports partly by the records of his own practice, partly by analyses of other writers:

1. That inflammatory and gangrenous affections of the skin not unfrequently have been developed and have repeatedly occurred in persons who for a considerable time had laboured under chronic melituria.

2. That sugar does not appear to occur in the urine of persons affected with the skin-diseases just mentioned, in whom diabetes does not exist.

3. That in rare cases of persons previously in perfect health, attacked by extensive carbuncular disease, which ran a tumultuous and fatal course, accompanied by septic symptoms, acute melituria has been observed.

In support of these views, Dr. Wagner brings forward an analysis of all the cases of inflammatory and gangrenous skin-affections which had occurred to him for fourteen months, amounting to fifty-two. In none of them was any sugar found in the urine. They were as follows:

	Males.	Females.
Carbuncle	5	3
Furuncle	9	6
Erysipelas	8	14
Erysipelas phlegmonodes . . .	2	5

Forty-eight of these recovered, four ended fatally.

Two interesting cases are given in detail, which bear out the writer's

third argument, and in which saccharine urine appeared in the course of severe and fatal carbuncular disease, attacking gentlemen previously in perfect health, and exhibiting no sign of diabetes.

ART. 76.—*Treatment of Diabetes mellitus.* By DR. INMAN, of Liverpool.

(*British Med. Journal*, Sept. 18, 1858.)

Two patients referred to in this communication are described as labouring men, about forty years of age—ill for many months. The quantity of urine passed was at least twenty pints daily, the specific gravity 1045; the presence of sugar was ascertained by fermentation and other tests. Emaciation was considerable, and thirst great. Both had been under dispensary treatment before their admission. In adopting a plan of treatment, Dr. Inman was guided by the following considerations:

1. The liver naturally produces sugar in a definite quantity. In diabetes there is an excess of sugar, and we may fairly infer that it comes from the liver. Opium has a decided effect in diminishing the bile-producing or secreting function of the liver, and it is reasonable to suppose that it will reduce the sugar-forming function. Experience has long told us that no single remedy in diabetes has been so efficacious in diminishing the quantity, &c., of urine passed, as opium. Opium, therefore, should be one ingredient in the treatment.

2. Again, Bernard has shown that the liver makes sugar, no matter what is the nature of the food employed. Dr. Budd has shown that some patients, at least, may be benefited by saccharine food. But these patients did not long for sugar; and they did enjoy their ordinary food; consequently the author neither restricted them to non-saccharine, or non-amylaceous diet, or prescribed unusual quantities of sugar. They were to have the ordinary full diet of the hospital, but more in quantity if they chose, either of bread, meat, or potatoes.

3. Again, it seemed to be clear that in diabetes there was debility, implicating more or less the whole system; that there was danger of death by consumption; that the digestive powers, notwithstanding their apparent energy, must be impaired; at any rate that opium was liable to disorder the stomach, and that it could be tolerated in larger quantity if combined with quinine.

The result of these considerations was the following prescription for a pill:

Opium, one grain; quinine, two grains: to be taken every four hours. Full house diet, with porter daily.

The effect of this was soon apparent. The men began to improve rapidly and steadily; the urine diminished until it stood at ten pints only per day, with a specific gravity of 1035. Commensurately with this, their strength and spirits increased, and they gained in flesh considerably. The opium never affected the head except on one occasion, when the patient, hoping to expedite his recovery, took a double dose. The bowels were habitually regular. The plan of treatment was neither varied nor altered during their residence in the hospital. They remained under notice, the one about three months, the other for six weeks. Both left the house of their own accord, as they considered

themselves sufficiently cured, and competent to do their ordinary work. Dr. Inman has seen one since he went out, and found that he continued strong, and, as he thought, well.

It is not imagined that these two cases are sufficient to upset our older notions of the correct treatment of diabetes. They are offered as a small contribution to our general therapeutical stores.

Dr. Inman mentions, as a curious fact, that one of his unsuccessful cases found that he received more benefit from a diet of raw beef than from any other thing, dietetic or medicinal, which he had taken; and that every new medicine did him good for about two days.

ART. 77.—*Sugar in Diabetes mellitus.*

By Dr. JOHN SLOANE, House-Surgeon to the Leicester Infirmary.

(*British Med. Journal*, May 29, 1858.)

In the first of these three cases the treatment was carried out for nine weeks, and the results were, an increase of three quarts in the daily quantity of urine, an increase of sugar, from fifty-six to eighty grains, in each fluid ounce, and an increase in weight of seven pounds. In the second case, which was treated for one month, the daily quantity of urine increased from eight to fourteen pints, the urine contained one grain more sugar in each fluid ounce, and the patient lost two and a half pounds in weight. In the third case the treatment was carried out for six weeks, and the patient lost four pounds in weight; the daily quantity of urine increasing by one pint, and each ounce containing an addition of four grains of sugar. Dr. Sloane, who has looked over the other cases in which this treatment has been tried, and finds ten, tells us, that in six the results of the treatment were injurious, and in four beneficial. He also tells us that the supply of drinks was limited (as M. Piorry says it ought to be) where the result was satisfactory.

CASE 1.—Nov. 22d. Elizabeth Lawrence, æt. 23, residing at Earl's Shilton, was admitted under the care of Mr. Paget on October 20th. She is a frame-work-knitter, of healthy parentage, and has suffered no privations. She has now a sister in the infirmary with diabetes mellitus. She is very temperate. She began to suffer from amenorrhœa three years ago; shortly afterwards her vision began to fail, and she thinks that for about the last year she has been passing an inordinate quantity of urine. She appears to be a delicate, badly developed girl, and seems younger than she really is. She has no pain, sleeps well, and feels very feeble. There is double cataract. The tongue is clean and moist; she has only eight teeth; the others "rotted away" about four years ago. The gums are congested, but do not bleed. She has much thirst; her appetite is extremely good; the bowels are regular without medicine. The lungs seem healthy. Pulse 72, rather small. There is no cardiac murmur. Not the least œdema of the feet or ankles is observed. The skin is dry and harsh. Her temper has been more irritable than formerly for the last six months. Hepatic dulness extended four inches and a half in a vertical line with the right nipple.

Since admission she has been taking five grains of citrate of iron three times a day, and has had the full diet of the infirmary, with a pint of beer and a mutton chop in addition. She passed during the last twenty-four hours five pints of urine. I examined a specimen passed five hours. It was clear,

like water, of acid reaction, of specific gravity 1040, and contained no albumen; chlorides were abundant. Each fluid ounce of this urine contained fifty-six grains of sugar, as calculated by Garrod's glucometer. She was ordered to take half a pound of treacle daily; no other change being made in her medicine or diet.

Dec. 15th.—During the last twenty-four hours she passed nine pints and a half of urine, nearly double the quantity she evacuated before she began to use the treacle. Each fluid ounce contained fifty-six grains of sugar, and the specific gravity was 1043. She had, however, gained three pounds in weight.

Jan. 28th, 1858.—She passed eleven pints of urine during the last twenty-four hours. The specific gravity was 1046; and each fluid ounce contained eighty grains of sugar. She had since last report gained four pounds and a half in weight. She omitted the treacle and the medicine also. She was ordered to take two teaspoonfuls of cod-liver oil and twenty-five minims of tincture of muriate of iron three times a day, and to have a pint of milk daily, in addition to the diet mentioned in my first report.

Feb. 28th.—She passed four quarts of urine in twenty-four hours. The specific gravity of some passed this morning is 1045; each ounce contains seventy-seven grains of sugar. There has been no change in weight since last report. Perhaps I should mention that, ten days ago, I examined a specimen of her urine, the specific gravity of which was 1042, and each fluid ounce contained sixty-three grains of sugar. She was ordered to omit her present diet, and to take instead half a pound of bran-biscuit prepared in accordance with Mr. Camplin's directions, tea without sugar, one pint of milk, three ounces of wine, a mutton chop, and one pint of beef-tea. No other article of food was allowed. Her medicine was continued as before.

March 2d.—During the last twenty-four hours she has passed five pints of urine. Some that she evacuated this morning had a specific gravity of 1046, and contained fifty-five grains of sugar in the fluid ounce.

April 9th.—She has continued to improve steadily and gradually since last report. She passed only three pints of urine in the last twenty-four hours, and each fluid ounce contains forty grains of sugar; specific gravity 1046.

CASE 2.—Nov. 24th. Ann Coleman, æt. 25, residing at Great Glenn, was admitted under the care of Dr. Crane, on September 15th. She is a servant, of healthy parentage, and of temperate habits. She suffered some privations when a child, but not lately. Four years ago she had an attack of typhus fever, from which she did not completely recover for twelve months. She was five months in bed, and for the remaining seven months was very weak. About twelve months ago she began to suffer from her present illness, and has since been gradually becoming worse. She is small of stature, and imperfectly developed, and appears to be much older than she really is. The cheeks are red; the veins of the face are unusually visible. She has no pain; sleeps badly; feels very weak; has observed no change in her temper; the special senses are unimpaired. The tongue is clean and moist; she has a good set of teeth. She has had no inordinate thirst for the last three weeks; previously, since her seizure, she had been very thirsty. Her appetite is very good—abnormally so. The bowels are regular. There is no cough; the lungs seem to be healthy. Pulse 72, small, feeble. There is no cardiac murmur. The skin is dry and harsh. There is no œdema of the feet or ankles. Hepatic dulness extends three inches and a quarter in a vertical line with the right nipple. She weighs five stone and five pounds and a half. The urine is clear, like water, of acid reaction, and of specific gravity 1040; it contains no albumen; chlorides are present. The quantity of sugar, calculated by Garrod's glucometer, is found to be seventy-six grains in each fluid ounce. She passed

four quarts of urine in the last twenty-four hours. She is taking daily half a pound of common biscuit, two eggs, one pint of milk, three ounces of wine, one pint of beef-tea, half a pound of light pudding, and the common diet of the infirmary. She has been treated with baths every other day at a temperature of 96° Fahr., and with five grains of compound soap pill three times a day. She believes that she has derived benefit from this plan. She was directed to continue her present treatment and diet, and to take in addition half a pound of treacle daily. After three days the treacle was discontinued, as it induced nausea, and she said it made her feel very thirsty. She was ordered to take half a pound of honey instead.

Dec. 8th.—The biscuit was omitted, and half a pound of bread substituted for it. She is now beginning to dislike the honey; but she was fond of it at first.

Dec. 20th.—During the last twenty-four hours she passed seven quarts of urine, being three quarts more than she voided in the same period before she began the saccharine plan of treatment. The specific gravity of the urine was 1043, and each fluid ounce contained seventy-seven grains of sugar. She has lost two pounds and a half in weight since the date of my first report, and feels much weaker since then, and is much more thirsty. She was ordered to omit the honey.

Dec. 25th.—The specific gravity of the urine was 1042; each ounce contained forty-five grains of sugar. She now began to complain of some cough. She was ordered to omit the baths and the opium, and to take two teaspoonfuls of cod-liver oil three times a day, in a dose of common cough mixture.

Feb. 20th, 1858.—During the last twenty-four hours she passed five quarts of urine, of specific gravity 1040; each ounce contains forty-eight grains of sugar. She is now very thirsty, and is gradually becoming weaker. She was afterwards ordered the bran biscuit as in the preceding case, but after two days she refused to take it. She was therefore discharged.

CASE 3.—Nov. 28th, 1857. Hannah Lawrence, æt. 14, residing at Earl's Shilton, was admitted under the care of Dr. Shaw on November 10th. She is a servant; has suffered no privations; is temperate; and has had no previous illness. She is sister to Elizabeth Lawrence, who is also suffering from glucosuria, and who is at present in this infirmary.

About three months ago she began to pass more urine than normal, and has since been gradually getting worse. She cannot account for her illness. She appears to be well grown, but is slender. She complains of pain in the left hypochondrium, at the margin of the ribs, in a line with the left nipple. It is sometimes present for an hour, and may be absent for two days; but these times are liable to vary much. She feels weak. The special senses are not impaired. The gums are not spongy; the teeth are good, two only being carious. The tongue is clean and moist. She has much thirst; her appetite is good—inordinately so as compared to the state of health. The bowels are regular without aperient medicine. She has had a cough for the last week; no lesion of lungs can be detected on examination; she raises no phlegm. Pulse 104, of moderate fulness and force; there is no cardiac murmur. The skin is dry and harsh to the touch. She had œdema of the feet for a week previously to admission; this has since disappeared. Hepatic dulness extends from an inch below the nipple downwards three inches and a quarter. She has never menstruated. Since admission she has taken Griffiths' mixture three times a day. She has the full diet of the infirmary, with half a pound of bread and three ounces of wine in addition. She weighs five stone and four pounds. During the last twenty-four hours she has passed nine pints of urine, which contains sixty-four grains of sugar in each fluid ounce, as calcu-

lated by Garrod's glucometer. The reaction of the urine is acid; the specific gravity 1040; some opacity, produced by heat, disappears on the addition of nitric acid; the chlorides are scanty. She was ordered to take half a pound of treacle daily, no other change being made in her medicine or diet.

Dec. 20th.—She passed during the last twenty-four hours the same quantity of urine as at the date of the last report. She has gained one pound in her weight. The specific gravity of the urine is 1045, and each ounce contains sixty grains of sugar.

Jan. 16th, 1858.—She has lost four pounds in weight since last report. The quantity of urine passed in twenty-four hours is five quarts; each ounce contains seventy grains of sugar; specific gravity 1035. She has become much weaker since admission. She was discharged at her own request. She died on February 17th, at her home in the country.

ART. 78.—*On the treatment of Diabetes by sugar.*
By Dr. WM. BUDD, Physician to the Bristol Infirmary.

(*Medical Times and Gazette*, May 22, 1858.)

This is the second case in which, with apparent benefit, Dr. Wm. Budd has tried this mode of treatment (*vide* 'Abstract,' XXVII, p. 105). The narrative of the case itself is prefaced by some critical remarks upon a paper by Dr. Bence Jones, which was noticed along with Dr. Wm. Budd's first case.

CASE.—Elizabeth H—, a widow, æt. 53, a diminutive person, and already for a considerable time the subject of diabetes, was admitted on the 11th of March, 1858, into ward 2 of the Bristol Royal Infirmary, where she still remains under treatment.

The symptoms which first arrested her own attention were frequent calls to make water, with great increase in the quantity passed, which amounted to several quarts daily; insatiable thirst, progressive weakness, and loss of flesh; and severe pruritus of the external parts. These complaints had come on rather suddenly about fifteen months before admission, in immediate sequel to severe mental anxiety. During the interval, she had been for some time an out-patient of the infirmary, and subsequently, and for a period of rather more than two months, a patient at the Clifton Dispensary. She had never before, within her recollection, had any illness requiring medical attendance.

When admitted, her debility and emaciation were extreme. The degree of wasting may be estimated by the fact, that when placed in the weighing-machine, she was found to weigh only sixty-five pounds. She was unable to stand, or even sit up in bed, without being supported. The skin was peculiarly harsh and dry; the pulse 100. On the day after her admission, and for many subsequent days, the tongue was dry and brown. Her nights were much disturbed by calls to make water, and her thirst was very great. There was a hectic flush on the cheek, and she was much harassed by frequent dry cough and by pains of the chest. Although there were no physical signs of lung-deposit, my impression was that she was most probably the subject of tubercle.

She had reached, in fact, what Dr. Prout describes as "the last and usually the briefest" stage of diabetes.

On the day after her admission, she was placed for five days on the ordinary (technically "the middle") diet of the house. No medicine was given.

During these five days she passed six pints of urine daily, of sp. gr. 1040°.

On March 17th she was ordered to take ten ounces of the best white sugar daily, and was put on the following dietary :

Bread (common), 12 ounces ; meat, 12 ounces ; butter, 3 ounces ; greens, 8 ounces ; beef-tea, 1 pint ; sherry, 4 ounces ; to all which two eggs were added on March 22d. An ounce of cod-liver oil, which she took daily from the day after her admission until April 16th, may also, I presume, be fitly included in this list.

On March 18th, twenty-four hours after the adoption of this scheme, there was an increase of three pints in the quantity of urine passed, which now amounted to nine pints, of sp. gr. 1040°. Her thirst had also increased, and she was weaker. The pulse had risen to 116. In short, she was in all respects worse.

On the 19th, under the same treatment, the quantity of urine fell to eight pints, and the specific gravity to 1037°.

As, with the exception of the sugar, the new dietary differed from that for which it was substituted chiefly in containing much less amylaceous matter, it was tolerably clear that the sudden aggravation of the diabetic symptoms was due to the administration of the sugar.

Under the idea that the fault might be, not in the principle, but in the application of it, I did not withdraw the sugar, but merely reduced its quantity from ten ounces to five.

As the patient complained much of inability to sleep, I, at the same time, ordered five grains of compound soap pill to be taken at night. I may add, that as this prescription seemed to answer, it was continued for the next ten days, when it was left off on account of constipation.

The opium probably had something to do with the sudden improvement which now occurred in the quality of the urine.

Be this as it may, the quantity of this secretion fell on the following day to five pints, its specific gravity continuing at 1037°. On the next succeeding day five pints were again passed, of specific gravity 1036°.

Between this date and the 30th March, the urine fluctuated in quantity between five and a half and six pints, and in specific gravity between 1037° and 1040°.

The only changes worthy of note in the patient herself were, that the tongue had become moist, and that she was somewhat stronger. She was able to sit up in bed, and could stand for a few moments without support. She relished the sugar, and it appeared to agree with her. Nevertheless, on being put in the weighing-machine on the 30th March, she weighed only sixty-four pounds ; so that in sixteen days she had lost one pound.

As from the history she gave of herself there was reason to believe that her loss of flesh for some time prior to her admission was more rapid than this, the true interpretation of this fact probably was, that some check was already being put to her downward progress.

In the next fortnight she steadily improved. On the 13th April, this note occurs in the journal of her case :

“ Weighed yesterday 69½ lbs. Pulse 76 ; tongue clean and moist ; skin soft and moist, having entirely lost its original harshness ; gains strength daily ; appetite good.”

She was now sitting up for an hour or two every day. The cough and other chest symptoms had entirely ceased. The only change made in the treatment during this last interval was, that on the 30th of March the sugar was increased to six ounces daily, and on the 3d of April a quinine mixture was ordered to be taken three times a day. Half a pint of bitter beer was

also substituted for the sherry. The urine meanwhile continued pretty steadily at six pints, its specific gravity ranging from 1035° to 1040°.

On the 20th—that is, seven days later—there is this further note: “Weight, 72 lbs.; better in every respect; remains up six hours every day, and walks about.”

In the week following she scarcely held her ground. In the early part of the week some relatives came to visit her, with whom she was on bad terms. A violent altercation ensued, and she was much agitated during the night. On the day following, the urine had increased more than a pint, and the specific gravity had risen several degrees. She continued to be very poorly for some days, and on the 27th she weighed only 71½ lbs., being half a pound less than the week before. Annoyances of the same kind have occurred to her several times since, and have always been followed by a similar change for the worse.

On the 24th the quinine was left off, and twenty minims of the dilute nitric and muriatic acids, in equal parts, were given three times a day instead. This last medicine she still continues to take.

On the 27th of April the sugar was increased to eight ounces—that is to say, to within two ounces of the quantity first prescribed. It is deeply worthy of remark, that without any other change being made, the urine on the day following, instead of increasing, showed a large diminution. On the 25th, 26th, and 27th, she passed six pints on each day, the respective specific gravities being 1035°, 1037°, 1034°. On the 28th of April she passed only five pints, of specific gravity 1034°. On the 29th, four and a half pints, of specific gravity 1035°; and on the 30th, again four and a half pints, of specific gravity 1034°.

On the 4th of May, her weight had further increased to seventy-three pounds—a point at which she still remains. Beside this, she is in all respects very much better. The tongue is clean and moist, the appetite is good, the bowels are regular, and the thirst is moderate. She is generally up the greater part of the day, and on fine days often passes much time in the infirmary garden. On the 3d of May, after having spent a good part of the morning in that way, she went out, on foot, to visit a sick relative, and walked more than half a mile without fatigue.

During the last ten days the urine has only once reached six pints, the average quantity for the other nine days being somewhat short of four pints and a half, and the average specific gravity for the whole period 1034°.

One change has occurred in the patient while under this treatment, which deserves to be specially mentioned, on account of its physiological interest. When she came into the hospital, she was almost as dark in complexion as persons affected with bronzed skin. As the treatment proceeded, she became visibly lighter, week by week, to such a point that it would be difficult to recognise her now as the same person.

The speedy subsidence of the chest symptoms equally deserves to be brought into prominent notice. We might, no doubt, attribute this to the opium employed. I would observe, however, that a precisely similar result occurred in the case of Snailum, to whom no opium was given. I withhold all comment from these facts at present, beyond suggesting that they may both very possibly be eventually found to possess an interest extending far beyond the present topic.

“In summing up the results detailed in the preceding narrative, the leading facts appear to be these:

“1. In the interval between the 30th March and the 4th May the

patient gained nine pounds (a gain representing one seventh of her whole weight), with a proportionate recovery of health and strength.

"2. During the same period, she was taking from five to eight ounces of sugar daily.

"And 3. Notwithstanding this large daily consumption of sugar, instead of an increase, there was an abatement of the proper diabetic symptoms.

"In connexion with these results, I shall confine myself at present to two remarks.

"The first is the very obvious one, that whatever may happen in other cases, or whatever may be the ulterior issue of this one, these facts will abide.

"The second is, that as it is impossible to suppose the sugar to have been without effect, the results here detailed must have been obtained either in consequence of its administration or in spite of it. If in consequence, then the facts are of extreme importance; but if in spite of it, they are scarcely less remarkable in the extreme opposition they offer to all we have hitherto been taught on the subject.

"In regard to this point, Dr. Prout, who is confessedly one of the highest authorities in these matters, uses these remarkable expressions: 'Every variety of the saccharine principle in its crystalline form is absolutely inadmissible as an article of diet in diabetes. This rule excludes, therefore, at once all fruits, whether subacid or sweet, as well as every compound, natural or artificial, into which sugar enters. The practical importance of this rule is so great, that I am doubtful, if it be neglected, whether good can be obtained from any plan of treatment.'*

"But here we have a diabetic patient eating from five to eight ounces of sugar daily, and not only rallying from a stage of the disease which Dr. Prout describes as being all but irretrievable, but adding in little more than a month a full seventh part to her weight, and becoming the while (what perhaps is most extraordinary of all) gradually less diabetic.

"Antagonism more extreme than this it is impossible to conceive.

"And there cannot, I think, be a more complete vindication than is to be found in it of the course I have taken in laying these facts in a simple manner before the public.

"I may add, in conclusion, that in publishing them I do so in the full confidence that they will be regarded by the profession generally as not without interest, in spite of attempts to disparage them, from whatever quarter they may come. Of this, at any rate, I feel very sure, and that is, that all such attempts will fail of their object when they come before us, seasoned by jests on homœopathy, having neither point nor good taste to recommend them.

"P.S.—Since the foregoing notes were sent to press, this patient has continued steadily to improve. Her weight now (May 18th) is 75 lbs. During the last five days, her urine has varied in quantity from 4 pints to $4\frac{1}{2}$ pints; in specific gravity from 1034° to 1037° . She is still taking eight ounces of sugar."

* 'Stomach and Renal Diseases,' by Dr. Prout, p. 39, 1848.

ART. 79.—*Inversion of the body for the symptoms produced by the passage of a Renal Calculus.* By Dr. SIMPSON, Professor of Midwifery in the University of Edinburgh.

(*Edinburgh Medical Journal*, July, 1858.)

At a recent meeting of the Medical and Chirurgical Society of Edinburgh, Dr. Simpson exhibited a small oblong renal calculus, from a patient who had passed such concretions at different times, and always suffered terribly during their transit from the kidney to the bladder. This patient had been now twice relieved from the agonising symptoms accompanying the passage of the calculus by inversion of the body. Professor Simpson had subjected her to this treatment in consequence of his belief that the passing calculus, falling down into, and becoming impacted in the ureter, acted at its point of arrestment as a pea-valve, and by its accumulating the urine above, or in the pelvis of the kidney and higher portion of the ureter, led to the accompanying distress by the morbid distension of these portions of the urinary ducts. When the body was inverted, and the affected side manipulated, the calculus probably fell backward, and consequently upwards, by its own gravity. At all events, whatever be the explanation, the practice in this and in one other case had immediately relieved the patient. He had seen partial relief from changed position in one case also of gall-stones. Position was a more important therapeutical agent than was generally supposed, not only in medicine, but also in surgery and therapeutics. Several years ago—and shortly after the famous case of Mr. Brunel—Dr. Simpson saw, with Dr. Paterson and Dr. James Duncan, a case in which a shilling passed into the wind-pipe, and where upon inversion of the patient the shilling fell back into his mouth, thus saving the patient from the operation of tracheotomy. Dr. Duncan had published a full account of the case. In prolapsus of the umbilical cord in labour, the mere gravity of the cord in the usual supine position of the patient was no doubt one great cause of the difficulty of retaining it in utero, above the head or presenting part of the child, when once returned. But some late cases and observations proved that the return and retention of the cord could be effected with comparative facility, if the aid of position was called in, and the patient was placed upon her face, or upon her hands and knees, till the presenting part filled the brim of the pelvis; for in this prone position the cord gravitated towards the fundus uteri, instead of towards the os.

(F) CONCERNING THE CUTANEOUS SYSTEM.

ART. 80.—*On a variety of Pellagra peculiar to the Insane.*
By Dr. BILLOD.

(*Archiv. Gén. de Méd.*, March, April, May, June, 1858.)

M. Billod bases this elaborate memoir upon fifty-six cases. Up to the present time it has been the habit to regard this affection as producing at a certain period of its course a form of delirium which has been

regarded by some writers as of a special character. In these cases, the mental symptoms take the initiative, and are regarded by M. Billod as the predisposing cause of the pellagra. We have some difficulty in understanding, however, that this should be an important cause of difference, for there can surely be no reason for supposing that insane persons are exempt from this affection; and if not exempt, then it follows that in them the mental symptoms will precede the others. These cases all occurred in the Asylum of Sainte-Gemmes, in persons who had been fed upon good wheaten bread, and consequently the affection cannot be ascribed to the use of maize. In a word, we do not think that this long memoir sheds much light upon the pathology of pellagra.

ART. 81.—*Ecthyma Simplex on the arms of Veterinarians from contact with the vagina of a cow.* By Mr. JOHN GAMGEE.

(*Edin. Vet. Review*, July, 1858; and *Medico-Chir. Review*, Oct., 1858.)

In the course of the protracted labour of a cow, Mr. Gamgee, Mr. Sarginson, a student of veterinary surgery, and a dairyman, had occasion to introduce their arms into the vagina of the animal, with a view to extracting the calf. All suffered from the appearance of a painful pustular eruption, which was particularly severe in the case of Mr. Gamgee, who describes it thus: on the evening of the day on which the ineffectual attempts were made to deliver the cow, he felt a strong itching sensation on the right arm. "My left," he says, "was not altogether exempt; and on looking carefully, I found that the skin was the seat of a diffuse rash. Next morning the redness had augmented on both arms, but chiefly on the right, and the pruriency was replaced by pain. On the Tuesday evening, an abundance of small circumscribed pimples had formed, which suppurated on the Wednesday. By the afternoon of the 25th they were well-formed pustules. The pustules were large, distinct, and surrounded by a red areola. Both arms were painful, the axillary glands slightly swollen, and I suffered somewhat in health. On the right the pustules were very numerous, exceeding one hundred, but less so on the left." A lotion of diacetate of lead and opium relieved the pain, "but some of the pustules attained the size of a large pea; others became encrusted with a brown scaly scab, and others, on bursting, left behind a cicatrizing sore. I was tormented for six weeks, when the only indication of the pre-existing eruption was the brown appearance of the skin where the pustules had existed." Some weeks later a carbuncle formed on the inside of the right arm, just above the elbow, which healed slowly. The description is accompanied by an excellent coloured plate, the original of which was by Mr. Cruickshank. Mr. Gamgee collects cases from various sources, showing that the occurrence of the pustular eruption under similar circumstances is not unusual, and he also quotes an instance that has been reported by Dr. Murphy, proving that similar contact with the human female may produce analogous results.

Mr. Gamgee regards the poison capable of producing such effects as of a distinct class, resulting from an exhausting process, both mental

and constitutional ; he compares it to the poison of puerperal fever or of dissection wounds. For his arguments we must refer to the paper itself.

ART. 82.—*Carate, a non-classified disease of the skin.*

By Dr. G. VAN ARCKEN.

(*American Medical Monthly*, April, 1858.)

The name of this disease, of which the author has failed to find any description in dermatological works, is derived from *cara*, the Spanish for *face*, and *ate*, the Indian for *look*—the word, according to Dr. Van Arcken, meaning, “Look at his face.” It occurs in New Granada and the northern parts of South America, and presents three varieties—the blue, the white, and the rose-coloured. The *blue* is the mildest, attacking persons of fifteen to twenty-five years of age, and consisting in the appearance of blue, round, or oval spots on the face. The spots coalesce, and extend down the neck on to the chest, where the ribs are often so distinctly marked as to cause the patient to resemble a zebra. The hands are a favorite seat of the disease, and again, the lower end of the tibia. It sometimes appears on the glans penis ; but the female organs of generation remain free. The *white* variety rarely occurs in the male ; it is commonly accompanied by diseases of the ovaries and uterus. The colour is of a dead chalky-white, and attacks people from thirty to forty years. The *rose-coloured* variety is the worst kind, and frequently follows the white, in which case there appears on the chalky spots of the latter some very minute red spots, which gradually enlarge, until the whole assumes a pale red colour. Those affected with this disease are mostly Sambos, Mulattoes, and others of a dark complexion. It always commences on the hands, extends from them to the face and neck, and then down over the abdomen. It attacks both sexes equally. Dr. Van Arcken attributes the disease to a combination of such influences as filth, exposure, syphilis, and insufficient and unwholesome food. Syphilis prevails so extensively, that scarcely one in a thousand inhabitants is free from it. It seems, therefore, to be a necessary complication of every disease occurring in those parts. Carate is regarded as incurable by the native practitioners ; Dr. Van Arcken has found alterative mercurial treatment, iodides, and arsenic, successful remedies. The blue variety he cures in about eight weeks ; the other two forms require a longer period ; “but the complete cases, whether they be congenital or contracted afterwards, are better left alone.” Carate is never fatal by itself.

ART. 83.—*On a peculiar Discoloration of the Skin in females.*

By Dr. BANKS, Physician to the Whitworth and Hardwicke Hospitals.

(*Dublin Quarterly Journal of Medicine*, May, 1858.)

This paper gives a resumé of the reports of all the cases hitherto recorded in which this strange discoloration of the skin was noticed, and relates another case which has fallen under the author’s own notice.

CASE.—An unmarried female, æt. 23, was admitted into the Richmond Lunatic Asylum on the 17th of September, 1853. Her history may be briefly stated: She is rather of a strong build, with dark eyes and hair, her face large, and without much expression. She is the daughter of a fisherman, and resided at the Skerries, a village on the coast, a few miles north of Dublin. She was engaged to be married to a young man who was hereditarily predisposed to insanity, and who committed suicide by hanging himself shortly before the time fixed for their marriage. She went to her lover's cottage, and, as she tells the story herself, his mother sent her into the room in which he was suspended; she knew something dreadful had happened, but did not know exactly what it was until, on rushing into the chamber, she struck suddenly and violently against his lifeless body. It appears that the fearful shock thus experienced had the effect of rapidly overturning her reason; she became maniacal, and the insanity assumed a suicidal propensity, her chief aim and object being to follow the example set her by her unfortunate lover. Having made repeated attempts at self-destruction, she was placed in the Richmond Penitentiary in the July preceding the date of her admission into the asylum. Soon after she came into the asylum, under the care of Dr. Mollan, she was attacked with erysipelas of the face, from which, however, she recovered in a few days. Her state, when first seen by Dr. Mollan, is reported to have been the following. She is restless generally, but at times sits quietly working with the needle, and talking to herself. She is most anxious to be permitted to go to a wood in the neighbourhood of her home, where she might dwell far from the haunts of man, surrounded by leafy trees, and listening to the songs of birds. On more than one occasion she became much excited, and used almost poetic language in giving utterance to her feelings. This, it may be observed, was the more strange, as she had received no education beyond what usually falls to the lot of persons of her rank in life. She evinces great anxiety to impress on all who come near her that she had not been sent to prison for any disgraceful crime, but avoids the subject of the real cause of her confinement. Her nights are disturbed and often almost sleepless, and she has several times endeavoured to cut herself with a pair of scissors. For the first two months there appeared to be little change in her mental state; the disposition to self-destruction continued undiminished, frequent attempts having been made to swallow pieces of glass, and to dash her head against the wall. In general she is engaged talking to herself, wrapt up in her own thoughts, and utterly regardless of all that is passing around her. After the lapse of some time she became more composed, generally melancholy, but occasionally almost cheerful. The catamenia have been irregular; the other functions have generally been performed in a healthy manner. For upwards of two years after her admission her state underwent little or no change. She was sometimes for three or four days excited, and then subsided into a quiescent condition.

Dr. Mollan, the senior physician of the asylum, who is at present at Cannes, has kindly favoured me with a history of this case during the period she was under his care. He informs me that after several months of amenorrhœa she became the subject of periodical hemoptysis, which was evidently vicarious of the menstrual flux. The hæmorrhage was generally attended with considerable excitement, often with great distension of the abdomen, and retention of urine. Dr. Mollan has frequently succeeded in preventing the attacks by leeching the groins, and by the use of the hip-bath two or three days before the expected period; he has, moreover, tried a great variety of treatment, including emmenagogues of all kinds, which have been administered without effect. In a communication which I have

lately received from him, he suggests that a trial should be made of galvanism, the current being passed through the ovaries and uterus. From a desire to watch the progress of the case for a short time, uninfluenced by treatment of any kind, I have hitherto abstained from putting into practice the suggestion of my friend and colleague Dr. Mollan, but, having now satisfied myself, I shall give the galvanism a full and fair trial.

On first seeing this young woman my attention was attracted by a remarkable discoloration around the eyes, but chiefly beneath and extending to the side of the nose; the appearance exactly resembles what one would imagine to have been produced by painting the part with Indian ink, or, perhaps, rather with Prussian blue. The discoloration around the eyes has existed since the period of the cessation of the catamenia, but it becomes of a more vivid hue during the continuance of the periodical hæmoptysis. The ordinary duration of the vicarious hæmorrhage from the bronchial tubes is from three to four days, and the quantity of blood lost is about what, in the healthy state of the system, might be expected to flow from the uterus. Shortly before the appearance of the hæmoptysis she grows restless and very much agitated; and all the time there is constipation, with a great amount of flatulent distension of the abdomen. Retention of urine also occurs, and it becomes frequently necessary to employ an instrument for its removal. The tongue becomes heavily loaded; and there is repeated and distressing retching, but seldom actual vomiting. During the retching it is observed that the discoloration around the eyes invariably becomes darker.

She speaks wildly and incoherently upon various subjects, but she ever reverts to the all-absorbing and dominant idea which has taken such painful possession of her mind. At these times she has often spoken to me of her lost lover, of his sad end, and of the shock it was to her, and how it suddenly bereft her of reason. She speaks with horror of his having died by his own hand, and of his having thus forfeited salvation; she also mourns over the disgrace of his not being buried in consecrated ground. She blames herself for not having married him, for she thinks he might have been saved. Her lamentations forcibly remind the listener of Bürger's ballad of Leonore. She is at all times most sensitive upon the subject of the blue discoloration of her skin, particularly when she is much excited; but even in the intervals she dislikes being observed, and I had some difficulty in persuading her to permit an artist to take her likeness. Once she said to me that she would rather her eyes were sightless than that she had the blue stains.

In addition to the interest connected with any case presenting the phenomenon under consideration, there is one peculiarity connected with the last, which renders it more than usually so, viz., the fact of the young woman labouring under mental alienation. The existence, also, of vicarious hæmorrhage of meteorismus, and of retention of urine, are worthy of especial notice.

ART. 84.—*A scarlet eruption caused by Hyoscyamus Niger.* By Dr. ROBERT CRAIK, Demonstrator of Anatomy in the University of McGill College.

(*Montreal Medical Chronicle*, Aug., 1858.)

The following case, which would seem to be unique, is of much interest, as showing that hyoscyamus has the same power as belladonna and stramonium of producing a rash like scarlatina.

CASE.—In July, 1857, I was called in great haste to see a child, two and a half years of age, who had swallowed some herbs which had been taken from the yard of the Montreal General Hospital. The father of the little girl described her as being “out of her senses,” and very much excited. Having observed a number of plants of *hyoscyamus* growing in the hospital yard, I suspected the nature of the poison at once. I sent the father home with a ten-grain dose of sulphate of zinc, with directions to administer it immediately on his arrival. I followed in about ten minutes, and found that the child had vomited slightly, the vomited matters consisting entirely of *hyoscyamus* seeds and capsules. The plant from which they had been taken was shown me, and proved to be a large one, not quite ripe, and having the whole of the capsules stripped from the upper part and probably swallowed by the child, so that nearly an ounce of capsules and seeds must have been taken.

The symptoms were so peculiar and so well marked, that poisoning by some one of the *solanaceæ* might have been diagnosed without any other evidence. There was the flushed and excited countenance, the restless and violent tossing, amounting almost to convulsions, the momentary listening to imaginary sounds, and the eager clutching at visionary phantoms; while the brilliant eye, widely dilated pupil, hurried pulse, and laboured respiration, filled up the pitiful but interesting picture.

One other symptom I must not omit, for it was among the most marked of all, and certainly not the least interesting. It was a bright scarlet redness of the whole surface, exactly resembling that of scarlatina. It was not a mere flushing of the surface, produced by the unusual exertion, but a well-defined papillary eruption, disappearing on firm pressure, but returning immediately when the pressure was removed. The mucous membrane partook to some extent of the same appearance as in scarlatina, though the strawberry tongue was of course not so well marked.

Another dose of sulphate of zinc having been administered without satisfactorily emptying the stomach, a teaspoonful of mustard was given, followed by copious draughts of warm water, which soon had the desired effect, very large quantities of the poisonous substances being evacuated. After recommending strong green tea as a drink, and applying cold to the head, I left her, promising to call again in two hours.

On my return I found the delirium and other symptoms still active, though not so violent as before, with occasional intervals of drowsiness. The eruption and the ocular delusions were as vivid as before.

I continued to visit her at intervals of a few hours during the night and following day, for the purpose of watching the decadence of the eruption, and I found that it, together with the ocular spectra, continued for about twelve hours from the time of taking the poison. Both then ceased gradually, and the child sank into a troubled sleep, interrupted by startings, twitchings of the muscles, &c., which did not entirely cease for upwards of twenty-four hours. The dilatation of the pupil continued for several days.

I watched the child carefully for some time, for the purpose of noting whether any attempt at desquamation would take place. On the fourth day numerous vesicles appeared on various parts of the body, resembling those of varicella. After remaining out for about two days they dried up, leaving scales, which peeled off along with portions of the surrounding cuticle. The thick epidermis of the hands and feet, however, showed no sign of desquamation. There was hoarseness and considerable irritation of the fauces for some days, probably partly due to the local action of the mustard, which we had some difficulty in compelling the child to swallow. In ten days the child was as well as ever.

PART II.—SURGERY.

SECT. I.—GENERAL QUESTIONS IN SURGERY.

(A) CONCERNING TUMOURS.

ART. 85.—*On the arrest of Cancer.*

By Mr. WEEDON COOKE, Surgeon to the Cancer Hospital, &c.

(*Medical Times and Gazette*, June 5, 1858.)

AFTER an exordium deprecatory of medical scepticism, which led to fatalism, and in praise of a just confidence in man's powers to heal his fellow man, "which," said the author, "is only bounded by his intellect, by his inability always to control the object of his treatment, and by time, which wears out the machine he works upon," Mr. Cooke referred to the different forms of cancer, scirrhus, epithelioma, medullary sarcoma, cystic sarcoma, &c., as having broad outlines too distinct to the eye and the touch to require often the delicate but sometimes illusive aid of the microscope. He then spoke of the recurrence of cancer after operation as so peculiarly characteristic of this disease that he was disposed to include in the same category all recurrent so-called benign tumours. After noting the peculiarities of the different forms of cancer, the author continues:—"In contemplating the phases of this disease, a glimmer of cheerfulness is shed over the sombre picture by a knowledge of the fact that scirrhus, the least active, is also the most common form. 1858 cases treated at the Cancer Hospital yield, of scirrhus 1344, epithelial 389, medullary 77, and cystic 35; so that scirrhus is as nearly three to one of all the other forms, and it is in this large majority that remedies are of most avail. The mamma is its most wonted seat, almost four fifths of the scirrhus cases occupying that gland." The period of life at which the disease appears corresponds to the cessation of the catamenia—the average age of attack being 44. The blood at this period is rendered impure by the absence of that customary process which in so marked a degree clarifies the system when it is in force. How the scirrhus is formed out of deteriorated blood may be imagined, but cannot be known. Even hereditary taint yields little towards the solution, since but one in seven can trace any predisposition. The fact,

however, of the climacteric association is still further shown in the Registrar's report for the last quarter, by which it appears that after forty years and upwards the deaths from cancer form four fifths of the whole number. On the other hand, the influence of local injury is only testified to in one in eight cases. The broad fact, therefore, stands out that in the very large majority of scirrhus cases they do develop themselves at that climacteric period when the vigour of life receives a severe shock, the effects of which it is the peculiar province of medical science to soften and subdue. For many years now it has been the custom to cut out this local manifestation of a general disease, and very recently to destroy the tumour by a cauterizing process, which has the peculiar advantage of prolonging the operation some forty or fifty days. Before the present century operations for cancer were not so rife, many eminent surgeons having opposed them when they were performed either by means of the knife or caustic, which latter was at that time enjoying an ephemeral reputation in France. Specifics then became fashionable; arsenic and hemlock enjoyed the highest reputation; but these proving unsatisfactory, early extirpation was again proposed, and has been most freely practised, encouraged, no doubt, by the introduction of anæsthesia by ether and chloroform. That this proceeding is a cutting of the Gordian knot, and not a skilful unravelment, cannot but be acknowledged, when we find that of 207 cases of operation observed at the Cancer Hospital, the disease has returned (taking the average) in fifteen months. A recurrence of the disease is more uncontrollable than the original tumour. A dyscrasia of the system is produced by the operation, and so greater activity of disease is provoked. By removing the tumour the reservoir is taken away, and the cancer-cells are distributed over the whole economy. "Experience," says Mr. Cooke, "leads me to the conclusion that there is in a great number of these tumours a natural ebb tide. The tumour grows at first rapidly, after a time, slowly; it then remains stationary, and at last begins to waste, until gradually it almost disappears. These favorable cases are seen in persons of cheerful temperament. In other persons the progress will be the same, but the termination a spontaneous sloughing out of the whole tumour. Sometimes the wound remaining will fill up and heal, or it will remain a dry scab for years without inconvenience, or a small superficial ulcer will continue to give some inconvenience, which may be relieved and kept in subjection for years by judicious treatment." Taking the climacteric disturbance as the grand exciting cause of the development of cancer, it is only rational to expect when that disturbance subsides, provided the patient is well supported and well managed, the mind relieved of all fear, and the cancer itself neither tattooed with caustics nor excised, that the cause being removed, the effect will have no further serious existence; that, in fact, the active cancerous tumour will become a mere inert fibrous mass, which is in a great measure reabsorbed by the neighbouring vessels. Numerous cases were read by the author, illustrative of his views, and showing the advantage of constitutional tonic treatment. He then dwelt upon the importance of the inculcation of hope—"that charm for every woe," by both surgeon and friends; "it medicines the body no less than the mind, and should be administered with a full conviction that it is a most valuable thera-

peutic agent, and not as an amiable delusion." He spoke also of the influence of temperament, and suggested the simple division of "cheering" and "desponding," instead of the old and more complicated arrangement of Hippocrates. It will invariably be found, that while a melancholy mind, in which hope cannot be raised, is the severest enemy with which the surgeon has to battle, a cheerful temperament is such an aid to art, that with it a sufferer from cancer will tide over the evil days, and live to the usual period of man's existence. The treatment most relied on was good nourishing diet, with beer or wine. Soda to correct the secretions if necessary. Bark and hydrochloric acid, or iron as a tonic. Iodine and mercury are positively injurious, and opium should never be resorted to, whilst there is any hope of restoring the patient. As a local application, lead, either as a supporting plaster or as a lotion, when there is much activity in the tumour, has a very sedative effect. The carrot poultice cleanses an ulcerated surface admirably, if frequently renewed, while the chlorate of potash lotion is the most effective in healing the open wound.

These and other simple methods of dealing with the disease, which are all founded on the principle of restoring and preserving, and giving support to the vital principle, wasting no time in looking after impossible specifics, will advantageously displace the use of the knife and caustic in the majority of cases; "although," said Mr. Cooke, "I am anxious to state that there are cases, *ex. gr.*, epithelial cancer of the lip and of the extremities, as well as some forms of medullary cancer, in which operation is desirable and beneficial." A letter from Mr. Cooper, Consulting-Surgeon to the Liverpool Infirmary, was quoted, in which he says: "For my own part, I confess that I have known few cases—scarce any—where the diagnosis has been unquestionable, in which extirpation has been successful. On the other hand, I have seen many cases, not interfered with by the knife, whose lives appeared little, if at all, shortened by the affection." In conclusion, the author affirmed, modestly but decidedly, that constitutional treatment was proved in its results to be immeasurably superior to operative treatment, so much so, that whereas the disease always returns after operation, and then with an increased impetus, by constitutional measures, which should include moral as well as physical support, and soothing local applications, it is arrested in its destructive progress, the new blood throws off the vicious habit of the old; no more cancer-cells are laid down. The old disease becomes a mere inert foreign body, which sloughs and is thrown off from the renovated system, or is gradually carried off with the other effete matters of the body. The plague is stayed, and the remainder of life may continue so uninfluenced by the terrible conflict which has been won, that length of days may be attained, and life ultimately ebb out in the calm twilight of evening.

ART. 86.—*On Cancer and New Growths.*

By Dr. WILKS, Assistant-Physician to Guy's Hospital.

(*Guy's Hospital Reports*, 3d series, vol. iv, 1858.)

Believing that the attempt to give some particular characteristics to one form of growth and name it cancer, and to another and style it inno-

cent, is only a partial way of regarding new growths, which amount to many in number, Dr. Wilks endeavours to take a general glance at morbid products of all kinds, and see how they pass by insensible degrees into one another. It may be true that each end of the scale of growths tends, in different directions, towards malignancy or innocency, or heterologous and analogous tissues, if these terms be preferred; that the former denote a vice in the system, and the latter a local perversion of nutrition. Dr. Wilks thinks, indeed, that the blood maintains in integrity the several tissues of the body, but that, if there be any local abnormal condition, arising, for example, from an injury, a so-called inflammation, with its products, results; and if the change in the part be chronic, a tumour may follow, the disposition still being towards the production of a tissue analogous to that near which it springs, the most complex probably being gland-tissue; but if there be some fault or vice in the constitution, the material thrown out cannot attain a highly developed form, but its tendency is to a rapid increase of a simple cell- or fibre-growth, which shall extend to a fatal result, although under some circumstances, as when near bone, the local tissue may be superadded, as in an innocent growth. Dr. Wilks also endeavours to show how inflammatory products can scarcely be distinguished from malignant or scrofulous, as in the peritoneum, or how in several cases an inflammatory product might with equal propriety be called a tumour; how in the skin these various products pass by insensible degrees into another, so that names can be only affixed to the most marked forms, and how the same holds good with the great number of new growths styled tumours.

ART. 87.—*On Fibro-plastic Growths.*

By Mr. BIRKETT, Surgeon to Guy's Hospital.

(*Guy's Hospital Reports*, 3d series, vol. iv, 1858.)

In this paper Mr. Birkett relates fourteen cases of fibro-plastic growths, and deduces the following conclusions:

"1. That the elementary tissues comprising the fibro-plastic growths differ from those entering into the composition of the tumours called carcinoma.

"2. That the natural history of the fibro-plastic growths is different from that of carcinoma.

"3. That the fibro-plastic growths may recur at the primary site of the new growth or in its immediate neighbourhood.

"4. That, unlike carcinoma, there does not appear to be a disposition to the production of fibro-plastic growths in any of the viscera of the chest or abdomen.

"5. That when secondary growths are developed in those organs, it will probably be carcinoma.

"6. That the glands of the lymphatic system do not become secondarily involved in disease, with the fibro-plastic growths, as they do with carcinoma.

"7. That amputation of a portion of a member will not in every case prevent the reproduction of fibro-plastic growth in the stump, even

although a joint intervene between the seat of the primary development and the stump.

"8. That excision of a primary fibro-plastic growth may be undertaken with a better chance of the eradication of the disease than follows the removal of carcinoma.

"9. That by the reproduction of a fibro-plastic growth, and as the result of changes taking place in the tumour itself, death may ensue without the viscera being affected by any organic disease.

"10. That the progress of the disease is slower than carcinoma; that is, that the time occupied by the development of the recurrent growths may extend over a very long series of years.

"11. That the fibro-plastic growths are developed at a somewhat earlier period of life than carcinoma.

"12. That they appear to be closely in relation with fasciæ, and very often to spring up in those parts of the body where the fascial envelopes or tendinous aponeuroses are developed in the most prominent manner.

"13. That they always form circumscribed lobes or masses, and never infiltrate the tissues of the organs of the body like carcinoma so frequently does.

ANALYTICAL TABLE OF CASES.

Age.*	Sex.	Site of primary growth.	Age of primary growth.	Excision repeated.	Survived first operation.	Survived discovery of disease.	Cause of death.†	Case.
30 to 40	M.	Thigh.	Some years.	I
27	F.	Neck.	14 years.	II
19	F.	Leg.	6 months.	Yes, and amputation.	III
15	F.	Thigh.	1 year.	17 times.	8½ years.	9½ years.	Pyæmia.	IV
30 to 40	M.	Scapula.	Some months.	7 times.	5 years.	6 or 7 years.	Pyæmia.	V
33	F.	Thigh.	5 or 6 years.	Twice, then amputation.	1 year.	6 or 7 years.	Exhaustion.	VI
13	F.	Thigh.	7 years.	...	5 weeks.	7 years.	Exhaustion.	VII
34	M.	Leg.	14 months.	Amputation.	VIII
48	M.	Leg.	18 months.	Amputation.	11 days.	18 months.	Pyæmia.	IX
35	F.	Leg.	6 years.	4 times, then amputation.	2 years.	8 years.	State of new growth.	X
24	F.	Neck.	2 years.	XI
20 to 30	M.	Back.	10 years.	Yes.	XII
13 or 14	M.	Forearm.	6 or 7 years.	XIII
50 to 60	F.	Diffused.	10 years.	Yes.	10 months.	11 years.	State of new growth.	XIV

* The age stated in this column is that of the patient at the time the disease was first observed.

† In this column, when not otherwise stated, the patient was alive and free from disease when last seen.

ART. 88.—*Treatment of enlarged Bursæ.*

By Mr. COULSON, Surgeon to St. Mary's Hospital.

(Lancet, May 8, 1858.)

"Reasoning from analogy," says Mr. Coulson, in a clinical lecture, "we might conclude that inflamed bursæ should be treated as inflamed synovial membranes, and indolent bursæ as encysted tumours. To a certain extent this may be true; but a great deal depends on the circumstances of each case. It is evident that active inflammation must be subdued by active means; but when this has been done, or when the tumour is naturally indolent, what treatment should we pursue? The principal methods recommended are—1. Rest and pressure. 2. Counter-irritation, mercurial or iodine frictions, &c. Abundant evidence exists to show that these and other similar means generally fail to effect a permanent cure. 3. Excision of the sac. In cases of small consolidated tumours this practice may sometimes be adopted; but extirpation of the bursæ is a severe operation; it has been followed by considerable inflammation, great disturbance of the health, and, in some cases, by death. 4. The bursal tumour, again, may be treated as a chronic abscess. It may be laid freely open by incision, or the contents may be evacuated, and iodine injections thrown in to modify the action of the lining membrane. 5. The practice, however, which I adopt myself, and which I would recommend to you, is more simple than any of the preceding; yet I have found it effective. The enlarged bursa is punctured with a grooved needle, such as is used for exploring tumours and swellings of a doubtful character. After evacuation of the contents, pressure is applied by means of soap-plaster and bandage. This is renewed from time to time, and puncture of the sac also repeated, if necessary. The result is generally a permanent and safe cure. Even in cases where the bursæ are inflamed, and the skin over them red, I should prefer the puncture now noticed to making any incision into the tumours or supposed abscesses. You will have observed that incisions were made by the house-surgeons in both the cases related to you, and that a considerable quantity of purulent matter was discharged through the wounds. This is not conformable to my practice. In one case it appeared to have answered well; but in that of Susan S—, the patient's life was very seriously endangered by extension of the inflammation to the joints and the neighbouring parts."

(B) CONCERNING WOUNDS AND ULCERS.

ART. 89.—*On the diagnosis and treatment of Syphilis in its primary forms.*

By Mr. HENRY THOMPSON, Assistant-Surgeon to University College Hospital.

(Lancet, July 3, 1858.)

Mr. Thompson commences this paper by demonstrating the importance of deciding promptly upon the nature of the primary forms of syphilis,

in relation to treatment and prognosis, and stated that our knowledge of syphilis had greatly advanced during the last few years, thanks to numerous observers in this country and abroad; but that to Ricord the merit is pre-eminently due of having defined the great laws which its phenomena exhibit. Without giving his adhesion in every respect to all the dicta of that illustrious observer, the author of the paper asserted that a careful examination of the subject compelled him to declare his conviction, that on almost, if not on all important points, his doctrines were supported by the phenomena of syphilis in this country.

Primary syphilis was defined as a specific disease communicated by a virus, of which the earliest manifestation is a chancre; and secondary syphilis as a constitutional affection, which, excluding hereditary transmission, originates always from a chancre, and manifests itself by characteristic symptoms, which follow, with more or less regularity, a certain order of evolution.

Two distinct varieties, and two only, of chancre were stated to exist—the soft or non-infecting, and the indurated or infecting chancre. Either of these might be attacked with phagedæna or sloughing, although much more commonly the former; but these conditions are the results of external circumstances, and not of any inherent quality in the sore itself. He laid down, as a principle, that, on seeing a sore in the early stage, we might, in five cases out of six, positively state to the patient, at the outset, a distinct prognosis as to the occurrence of secondary symptoms or the contrary, without risk of error; and that, in consequence, we might select the appropriate treatment at once, and pursue it with confidence.

Mr. Thompson defined the external characters of the indurated or infecting chancre; contrasted them with those of the soft or non-infecting chancre; pointed out that the first was *invariably* attended with indurated, painless lymphatic glands in the groin, which attested the nature of the sore after the latter had disappeared; and stated that constitutional syphilis was certain to follow sooner or later, the induration of the sore itself being, in fact, the first sign of the systemic infection. Next, he described the character of the soft chancre, which was not necessarily, nor, indeed, most commonly, associated with any bubo at all, but if so, the bubo was inflammatory and would suppurate. In this case it was almost certain that secondary symptoms would follow.

He then considered the sores of a doubtful character, that is, those respecting which it was difficult at first to determine the nature, and showed how the two varieties might, nevertheless, in most cases, be distinguished by attention to known causes of error.

The treatment of primary syphilis, in these two forms, then succeeded. The employment of caustic, which, if sufficiently powerful, and applied early, would prevent constitutional infection, was strongly recommended. The Potassa cum Calce, on the whole, was regarded as the best. In the soft chancre, which was met with three or four times as often as the indurated chancre, there could be no occasion for mercury and iodine, as it was a purely local, not a constitutional disease. Local astringents or antiseptics, and if it was slow to heal, fifteen or twenty-grain doses of the potassio-tartrate of iron, twice or thrice a day, formed the best

treatment. Such formed the bulk of the cases so frequently reported as examples of syphilis cured without mercury; in fact, whatever the treatment of these sores, no constitutional symptoms would manifest themselves. In the well-marked indurated chancre, small doses of the iodide of mercury, such as three quarters of a grain or a grain, guarded by about two grains of Dover's powder, appeared to suit more generally than any other form. The gums to be but very slightly touched, and the patient carefully preserved from salivation; this condition to be maintained for a considerable period. Where any intolerance of mercury by mouth was exhibited, inunction or fumigation should be substituted. Nothing, however, could be more obvious than the good effects of mercury in these truly infecting sores and early constitutional symptoms, provided its administration be kept within the limits recommended.

A tabular form, exhibiting the characters and tendencies of the two varieties of chancre by way of contrast, was presented, for the purpose of diagnosis, and showing the salient points of the subject at a glance. A copy of it follows here:

Diagnostic characters of the two varieties of venereal sores.

1. *The soft or non-infecting chancre.*
2. *The indurated or infecting chancre.*

THE SOFT CHANCRE.

Anatomical characters.—Form: rounded, often irregularly so. Edges: sharp, well defined, as if cut with a punch; rather overhanging; not adhering closely to subjacent tissues. Surface: flat, but irregular, "worm-eaten;" often with yellowish or grayish matter adhering. No induration of tissues around, unless caused by caustic or other irritant; in which case the thickening is not defined in its limits, but shades off into the surrounding tissues, and has more or less the aspect of inflammatory action.

Pathological tendencies.—The secretion is contagious, purulent, and plentiful; hence these chancres are rarely single; often, perhaps most commonly, multiple, one giving rise to another. It is usually slow to heal, has a tendency to spread, and is liable to take on phagedænic action. The soft chancre appears, from the records of practice, to occur with a frequency about four times as great as the indurated chancre.

Characteristic gland-affection.—In many cases (but not in the majority) the inguinal glands are affected; in which case, one gland, usually, rapidly inflames and suppurates, and an open bubo is the result. The pus, at first, is inoculable, and capable of producing a soft chancre.

Prognosis.—The well-marked soft chancre is always a local affection, and does not affect the system; and no "specific" treatment (mercury and iodine) is required.

THE INDURATED CHANCRE.

Anatomical characters.—Form: rounded. Edges: sloping, not sharply

cut; hard, sometimes a little elevated, closely united with subjacent tissues. Surface: hollowed or scooped out, but smooth, as if varnished; often grayish at the centre. Induration well defined, incompressible, like a cup of cartilage let into, or set upon, the tissues beneath, and movable over them; no inflammatory areola; usually makes its first appearance between the fifth and tenth day, never after the twentieth; generally long survives ulceration. Induration varies in degree somewhat with the situation; but, when slight, is nevertheless always defined.

Pathological tendencies.—The secretion is scanty, rather serous than purulent, and is not very readily inoculated; hence the sore is usually single, rarely multiple, and if so the sores appear simultaneously. It is indolent, but less so perhaps than the soft chancre; rarely takes on phagedæna. Either sore propagates by inoculation; invariably produces its like.

Characteristic gland-affection.—It is *invariably* followed by slight swelling and marked induration of the inguinal glands on one or both sides (the sore being on the genital organs); usually several glands are affected; they are hard, incompressible and roll under the finger, are painless, and do not inflame or suppurate; except, in rare instances, from over-exertion, in scrofulous subjects, &c., but then the pus is not specific and not inoculable. The induration of the gland coincides in time with that of the chancre itself. The primary sore having disappeared, or being denied, the gland-induration is an invaluable sign for purposes of diagnosis.

Prognosis.—Constitutional syphilis will certainly declare itself sooner or later. Mercury will retard, modify, or prevent the evolution of secondary symptoms.

(C) CONCERNING FRACTURES AND DISLOCATIONS.

ART. 90.—*On the reproduction of Bones.* By Dr. TOLAND.

(*Charleston Med. Journal and Review*, July, 1858.)

In this paper Dr. Toland endeavours to show that entire bones and joints may be restored by proper management.

"In 1853," says Dr. Toland, "when I took charge of some wards in the State Marine Hospital, I found in the fourth ward, No. 12, a Mexican who had long been suffering from caries of the inferior maxillary bone. Finding the bone destroyed anteriorly, and the remainder diseased, incisions were made on the inside of the mouth, and the entire submaxillary bone removed. In a few weeks bony matter was deposited, and the motion of the jaw perfect. He left the hospital but little disfigured, and if the teeth had not been lost, the reproduced jaw would have been as perfect as the original. Dr. Reilly was then the resident physician, assisted in the operation, and witnessed the result.

"I was much gratified to find, in the July number of the '*Charleston Medical Journal*,' a case reported in the '*Moniteur des Hôpitaux*,' by Maisonneuve, of a similar character: 'The entire lower jaw was excised

for an enormous fibrous tumour developed upon and involving the bone. In extracting the jaw, the periosteum was left *in situ*. A rapid cure was obtained—nearly the entire incision healing by the first intention. So little deformity results, that it requires a practised eye to detect the absence of this important bone. The movements of the mouth are all preserved; the tongue has recovered all its movements; speech is clear and distinct; swallowing is effected with great facility, and one month after the operation a dense substance was forming from the periosteum, which was expected in time to form a useful bony mass.’

“In 1853 a millwright, from Contra Costa, was admitted into the State Marine Hospital with a comminuted fracture of the great toe of the right foot. Finding, at the expiration of a month, that the bones were diseased, and believing, from the result of the treatment in the former case that the bones would be restored, instead of amputating the toe, they were removed. In four weeks they were reproduced, and the toe was as useful as before the injury—although I was not aware that the articulations were restored, supposing that a ligamentous substance supplied, imperfectly, the place of joints. Dr. Reiley assisted in this operation.

“In June, 1856, James Clark, who lives near the corner of Filbert and Battery Streets, consulted me respecting the propriety of having the middle finger of the right hand amputated. He had been under the care of a physician, who thought its removal necessary.

“The first and only half of the second phalanges being diseased, and believing they would be restored, I advised their removal; for if the first joint was even destroyed, the finger would still be useful. Assisted by Mr. Lindop, an incision was made from near the junction of the second and third phalanges, and extended under the nail to the same point on the opposite side. The soft parts were then dissected from the bones, and the second phalanx divided about a quarter of an inch anterior to the joint. The wound was then closed by the interrupted suture, and the soft parts retained in a proper position by pasteboard splints and a bandage.

“In four weeks the bones were not only restored, but a joint had also been formed, the motion of which was as perfect as the original. Being a porter, and having used the finger before the ligaments were sufficiently strong to resist the force applied, there is a slight lateral curvature; but, in every other respect, the finger is as useful and perfect as the one on the other hand.

“Mr. M’Gowan, employed at Newland’s stable, on California Street, was advised in June, 1856, to have the forefinger of the right hand removed at the second joint, and he came to my office for the purpose of complying with instructions. Instead of amputating the finger, the first and second phalanges were removed, and then treated as the preceding case.

“In a few days he resumed his occupation, and I did not see him for several months. His finger has recently been examined; the second joint is perfect, and the entire phalanx restored. The first phalanx, in consequence of the soft parts being allowed to contract, is shorter than the original—although a joint exists, and the finger is as strong and useful as before the operation.

"Mr. Shannon, a cooper, who resides at No. 6, Jackson Street, had a whitlow on the right forefinger, involving only the first phalanx, which was removed by a lateral incision. He resumed his business in a few days after the operation, and, although not subjected to the proper treatment, the bone was reproduced, and the motion of the joint is as perfect and the finger as strong and useful as before.

"Mr. Collins, who lives in this city, on Stevenson Street, between First and Second, had suffered for three months from a whitlow involving the whole of the right thumb. Before he became my patient, free incisions were made without affording relief. The thumb was enormously enlarged. The first and second phalanges were diseased, and the flexor tendon near the extremity destroyed.

"Notwithstanding its excessively diseased condition, I determined to remove the bones, although confident that the thumb would not be as useful and perfect as it would have been if he had received proper attention at an earlier period.

"On the 1st of April, 1857, the first and second phalanges were removed by a single incision, and the wound closed by the interrupted suture. Pasteboard splints and a bandage were then applied, and continued until cured.

"In six weeks the soft parts were healthy, and the bones and joints restored. Notwithstanding the destruction of a portion of the tendon, he has control even over the first joint, and the strength and motion of the thumb are daily increasing, which would not be the case if the tendon had not been reproduced.

"Mr. Littlejohn, who resides at Redford City, had suffered greatly for several weeks from a similar difficulty. He was operated upon on the 18th of April. An incision was made on the external side of the thumb, and both bones removed. This case progressed much more rapidly than that of Collins—and in four weeks the bones and joints were reproduced. The thumb is now as strong, and the motion as perfect, as before the occurrence of the difficulty.

"Mr. Blaisdell, who now resides in San Francisco, had the great toe of the left foot injured by a stick of timber. When examined, I found the second phalanx carious, although the first was healthy.

"In December, 1857, an incision was made upon the external side, extending from the articulation with the metatarsal bone to the extremity; both phalanges were removed, which was unnecessary, and the wound closed as usual. In four weeks from the time the operation was performed he could wear a boot without inconvenience. Both bones, with joints, have been restored, although the first phalanx was not diseased, which can only be accounted for by supposing that the periosteum detached from the second phalanx furnished a sufficiency of bony matter for the restoration of both.

"Elizabeth Gallman, who lives with Mr. Grey, North Beach, had suffered for three weeks from a whitlow of the right middle finger. The first phalanx was removed in presence of Dr. Raymond, and the finger examined in a few days by Drs. Sheldon and Hewer, which, although slightly enlarged, is as useful as before the operation, and the motion of the joint as perfect.

"Mrs. Cunningham had a needle broken in the first joint of the right

forefinger, which produced caries of the first and half of the second phalanges. On the 1st of September, 1857, they were removed in presence of Dr. Hewer. In five weeks she could use a needle with facility; and now, with the exception of a slight diminution in the length, the finger is perfect. She lives on Yerba Buena Street, and has recently been examined by Drs. Fitch and D. L. D. Sheldon.

"Recently, in the case of Mrs. Stone, of Sacramento, one third of the second phalanx and two thirds of the third were diseased, and the remainder healthy. The diseased bones, including the second joint, were removed in presence of Drs. Fitch and Hewer. Fifteen days have elapsed since the operation was performed, and the case is progressing favorably.

"Believing that the same course of treatment would be found equally successful when longer bones are implicated, during the summer and fall of 1856, assisted by Dr. Lind, the resident physician of the U. S. Marine Hospital, more than half of the clavicle, the os calcis, and portions of the os femoris and tibia, were removed with a similar result; but being unable—as they were sailors—to specify their place of residence, their cases will not be given in detail, although they were as successful as any that were previously or have since been subjected to the same treatment.

"On the 3d of December, 1857, James Allen, a gentleman from Crescent City, was examined, who had received a gunshot wound, ten months before. The ball passed through the wrist, and he had suffered excessively. Finding all the carpal bones and those of the wrist, with which they were connected, extensively diseased, I determined to remove them, it being the only course of treatment which could save the hand. Assisted by Drs. Fitch and Hewer, an incision was made in the direction of the metacarpal bone of the forefinger, which was removed, with half of the metacarpal bones of the other fingers, as well as the trapezoides, magnum, unciforme, and pisiforme. But one incision was made in the soft parts, and great care taken not to divide either the tendons or large blood-vessels. He has suffered but little since the operation. The constitutional disturbance is considerable; the hand is not swollen; the wound presents a healthy appearance, and no doubt is now entertained of saving the hand, and restoring it to usefulness.

"Although I have been engaged in the investigation of this subject for several years, even my personal friends were not apprised of the fact, until a sufficient number of cases could be presented to remove all doubt upon the subject. I am now satisfied that the proper course has been pursued, for the above cases not only prove the restoration of bone, but also that articulations sufficiently perfect have been and will invariably be reproduced."

(D) CONCERNING DISEASES OF JOINTS.

ART. 91.—*The restoration of Motion by forcible extension and rupture of the uniting medium in partially anchylosed surfaces.* By Mr. BRODHURST, Assistant-Surgeon to the Royal Orthopædic Hospital.

(Pamphlet, Adlard, 1858.)

In a former volume ('Abstract,' XXV, p. 146) we gave an abstract of a paper in which Mr. Brodhurst recounts several cases of partial anchylosis of the hip, knee, and elbow-joint, in which the uniting medium had been ruptured, and where, after a varying amount of time, the power of motion had been successfully and safely established. In the present pamphlet some additional cases are recorded, together with some further details as to the operation itself. Thirty-two cases have now been operated upon, and this is the result: In eleven instances complete power of motion, or nearly complete power, has been gained; in fourteen, partial, but useful, motion has been restored; and in seven, the limb has been rendered straight, and the joint has remained stiff. Of the eleven first-mentioned cases, eight were of the hip, one of the shoulder, one of the elbow, and one of the ankle. Of the fourteen in the second series, five were of the knee, four of the hip, two of the elbow, one of the shoulder, and two of the ankle; and of the remaining seven, four were of the knee, one of the hip, one of the ankle, and one of the elbow.

"The affections of joints," says Mr. Brodhurst, "to which this operation is applicable may be divided into two classes: namely, those in which muscular contraction coexists to so great an extent as to prevent the application of force to the adhesions until tendons and fasciæ have been subcutaneously divided, and those in which the adhesions may be easily ruptured, on moderate force being applied, and without the previous section of tendons, fasciæ, &c.

"In cases of partial anchylosis some muscular rigidity exists. About the hip-joint it may prevent all semblance of motion: the extremity may be in its extended position, with the joint apparently rigidly fixed. At the knee-joint, also, the flexor muscles of the leg may be so rigidly contracted as to prevent the slightest motion of the joint. It is more common, however, to find slight motion (just appreciable motion) at the knee, even with great contraction of the flexor muscles.

"Under the influence of chloroform, muscular rigidity may in some instances be so far overcome, that the adhesions may be ruptured without previous subcutaneous division of soft structures. Often, however, it is necessary to divide the tense tendons before the adhesions can be reached. Now, it is evident that, if force be applied to overcome great muscular contraction, and at the same time to rupture the fibrous adhesions, the soft structures—namely, the muscles—must yield before the deeper adhesions can be reached. But this is the treatment which was pursued by Louvrier, and was subsequently discarded on account of the fatal results which were induced by excessive violence. He applied so

much force, and so suddenly, by means of an instrument which had been constructed for this purpose, that muscles were lacerated, arteries were torn through, and bones were fractured. Hence it is that I have divided these cases into two classes: namely, those in which, from great muscular contraction, it is necessary to divide tendons and fasciæ, and subsequently to rupture the adhesions; and, secondly, those in which muscular contraction may be sufficiently overcome by the exhibition of chloroform, to enable the adhesions to be ruptured without the use of the knife.

“When tendons have to be divided, the punctures are to be allowed to heal before extension is made. Dieffenbach, who was among the first to direct attention to this question, divided the tendons subcutaneously, and immediately afterwards extended the limb forcibly.* The wounds were consequently made to gape; and they thus became starting points for extensive lacerations of the integument. Some of his operations were, doubtless, so far successful, that a crooked limb was made straight; but others were followed by violent inflammation and extensive suppuration, and in some instances it was necessary to resort to amputation. In none was the motion of the joint restored.

“And, again, Langenbeck taught that the employment of anæsthetic agents caused perfect relaxation of the contracted muscles; so that the patient, being under their influence, it was not necessary to divide tendons, for the limb might then be extended without fear of rupturing the muscles.† That this statement involves an error was proved by his practice; for serious accidents—such as dislocations—not unfrequently attended these violent operations. Besides, when structural change has taken place in the muscle—when it has been for a long period much contracted, or when adhesions exist—it cannot yield to a suddenly extending force without its fibres being ruptured.

“Therefore, it is manifest that it became necessary to reconsider the treatment of partial ankylosis, that neither the danger which had been incurred by Langenbeck should again be encountered, nor the more serious consequences which had resulted from forcible extension with gaping wounds, as had been practised by Dieffenbach, Palasciano, Bonnet, and others.

“It has been my first object in the treatment of these cases to remove such impediments to extension as are offered by contracted muscles and by tense fasciæ—to divide subcutaneously all such structures as would be likely to interfere with the extending process. The wounds were then closed, and reunion was promoted by rest. When this had fully taken place, the full effect of chloroform was obtained, and the limb was extended, a suddenly imposed force or a series of jerks in the direction of flexion being sufficient to cause the adhesions to yield suddenly with a snap or with a more prolonged tearing sound, or, indeed, without an audible result, and with sudden yielding. But when muscular tension could be entirely overcome by chloroform, and the condition of the limb was such that subcutaneous sections were not necessary, there being neither tense fasciæ nor adhesions of the skin, chloroform was admi-

* Durchschneidung der Sehnen und Muskeln.

† Commentatio de Contractura et Ancylosi Genu.

nistered, and its full effect having been obtained, the adhesions were ruptured, muscular relaxation being complete.

"If one point is more worthy of attention than another, it is the management of the skin while the fibrous adhesions are being ruptured. Adherent cicatrices and points of adhesion should be previously subcutaneously divided; so that unequal pressure may, as far as is possible, be removed during the act of extension, and especially from those weakest points—the neighbourhood of cicatrices. And should the continuity of the integument be endangered by the extension which may be necessary for the replacement of the articular surfaces, it is preferable to complete this replacement on a second occasion rather than to risk the smallest rent of the skin. As might *à priori* be expected, those cases are attended with the greatest success where the adhesions are ruptured on the application of moderate force, and when they yield with a single snap; where the skin is in no measure endangered; where the adhesions are extra-capsular; and where the integrity of the joint is so far preserved that there is no tendency to dislocation. When, however, in consequence of partial dislocation, of extensive adhesions within the joint, or from other cause, considerable force has to be employed, it behoves the surgeon to be careful as to the direction and extent of the force used, especially when cicatrices exist, that the integument may not, by a violent movement of the limb, be ruptured. With care this accident will never occur. But, as it is not always possible to destroy all the existing adhesions without endangering the continuity of the integuments, it is more prudent, when great tension has been induced and rupture of the skin appears to be imminent, to remit extension, and to complete the operation on a future occasion. After the subsidence of any inflammation or tenderness which may have been induced, the remaining adhesions will probably yield to gentle pressure, or on the application of slight force.

"Again, the following is a point to which I would especially direct attention, namely, that the adhesions having been ruptured, no further motion or examination of the joint should be permitted. The observance of this rule is, I believe, essential to ultimate success. On one occasion I disregarded it, and, having ruptured the adhesions, I examined the joint to ascertain that its motions were extended and perfectly free. Inflammation followed, which lasted several days. It is unnecessary to examine the state of the joint at this time. The surgeon may rest assured that the joint is free when he has heard the snap, or when he has felt the limb suddenly yield. His whole aim then should be to prevent inflammation, which is most certainly effected by preventing any further motion of the limb.

"Before chloroform is administered, a gutta-percha splint should be moulded to the limb; and it may be allowed to harden while the chloroform is taking effect. This splint is to be removed before extension is made, and it is to be replaced as soon as the adhesions have been ruptured, and it may be worn until tenderness about the joint has ceased. When tenderness has entirely or nearly disappeared, the limb may be moved gently. It may be necessary to give a small quantity of chloroform before motion is attempted for the first time, or even on two or three subsequent occasions. Each time motion will be borne more

easily than the last time, and very soon forcible movements by means of ropes and pulleys may be instituted. Or, in the case of the hip, if the pelvis cannot be otherwise firmly fixed while the thigh is being moved, a chair should be constructed by means of which the pelvis may be firmly grasped, and the thigh may then be flexed and extended to the full extent of motion, or as far as it can be borne, by means of a lever attached to the chair itself. Also, when the shoulder is the affected joint, some difficulty may be experienced in fixing the scapula firmly. This, however, may be overcome by means of a gutta-percha splint moulded accurately to the upper part of the back, the ribs and the lower part of the neck, leaving the joint itself uncovered. When this is firmly bandaged on to the thorax, motion may be given to the upper arm without fear of moving the scapula.

“After the tendons have been divided, and the adhesions have been broken down, an extending apparatus is to be applied, and extension is to be carried on rapidly. And when extension is complete, motion is to be attempted, at first under the influence of chloroform, and passive motion may then be continued, as has been above explained.

“In some instances after rupture of the adhesions, pain is inconsiderable, and passive motion can be borne well; but in others motion cannot be borne, or it cannot be borne with sufficient force to re-establish the use of the joint. It is essential that passive motion should be employed during many weeks, not only that the joint may reaccommodate itself to motion, but also that, where the adhesions are intra-articular, the joint may be, so to say, redeveloped, just as a ‘false’ joint is formed by solution of the solid fibrous material connecting the broken ends of a long bone, where perfect union has been prevented by motion of the parts. Also, passive motion is essential, that the muscles which have been long motionless may lose their rigidity, and that, from being attenuated and pale, they may regain their fulness and colour. The time which is necessary to this end varies, and is in some measure proportionate to the period during which these organic changes have existed. A short time, however, suffices in a large number of instances to regain some power of motion; and when power of voluntary motion has commenced, it increases rapidly. Even in the ‘false’ joint a capsule may be formed, and synovia may be secreted; how much more readily the structures will reaccommodate themselves in the true joint to the purposes of motion will be shown by some cases which I have selected out of many, as examples of the restoration of motion in joints after the rupture of adhesions.

“But, it may be asked, to what class of cases is this operation applicable? To which I would reply that it is applicable to all forms of partial ankylosis which have resulted from simple forms of inflammation. As, however, various meanings are attached to the terms true, and false, or partial ankylosis, it will be desirable to give an exact definition of the terms, that it may be fully understood what meaning is intended to be conveyed.

“True ankylosis is said by some writers to consist of intra-capsular adhesions, and false ankylosis of extra-capsular adhesions. Others describe true ankylosis as ‘loss of motion in a joint,’ and false ankylosis as ‘that condition in which the movements of the joint are

more or less interfered with.' And, again, a third definition is, that bony adhesions constitute true ankylosis, while fibrous adhesions or muscular rigidity only, form partial, or false ankylosis. Therefore, it is necessary to define the meanings which the terms 'true' and 'false' are intended to convey.

"There is no doubt that the term 'ankylosis' may be applied to rigidity of a joint, from whatever cause. In using the term 'false ankylosis,' however, I wish to be understood as referring to that condition of a joint in which fibrous adhesions have been formed between the articular surfaces entering into the formation of the joint, or which connect the extremities of the bones—whether intra- or extra-capsular, in contradistinction to bony, or true ankylosis. And without limiting the term 'false ankylosis' to fibrous adhesions within or without the joint, I wish for my present purpose to understand by the term false ankylosis fibrous adhesions within or external to the capsule, producing immobility of the joint."

(E) CONCERNING OPERATIONS.

ART. 92.—*A new method of Amputating.* By M. MAISONNEUVE.

(*Archiv. Gén. de Méd.*, June, 1858.)

At a meeting of the Académie des Sciences, held April 26th, 1858, M. Maisonneuve read a note on a new operation for amputation, which he calls the *diaclastic method*. The peculiarity of this method is, that the knife is not used for dividing the muscles, nor the saw for cutting the bones, nor permanent ligatures to arrest hæmorrhage; and that, contrary to the ordinary methods, the division of the bone constitutes the first step of the operation, and precedes the division of the soft parts.

The principal object of this method is to avoid the occurrence of purulent infection, by substituting for the ordinary process of division by cutting instruments, the process of breaking, tearing, and extemporaneous ligature, the contusing action of which obliterates effectually the vascular orifices.

M. Maisonneuve uses for the execution of his method an *osteoclast*, or instrument for breaking the bone; and a powerful *serre-nœud* for the division of the soft parts. The author describes the operation in the following manner:

The patient having been brought under the influence of chloroform, the surgeon applies the osteoclast precisely on the spot where he intends to break the bone, taking care to protect the soft parts in contact with the instrument by thick compresses; then, giving the screw several turns, he produces the fracture; he removes the instrument, and immediately substitutes the serre-nœud, in the metallic loop of which he embraces the member ten or fifteen centimetres below the point of fracture; then, turning the screw, he gradually compresses the tissues until the circulation in the vessels is interrupted. This done, he divides the muscles to the bone by a circular incision with the bistoury, two or three

centimetres below the *serre-nœud*, tears off by a twisting movement the extremity of the member which is attached merely by some adhering portions of muscular tissue, and finishes the operation by continuing to turn the screw of the *serre-nœud* until the tissues embraced in the loop of the ligature are completely divided. If the last step of the operation is conducted with prudent slowness, not a drop of blood will ooze from the wound resulting from the amputation, whatever the amputated member may be.

This singular method has been applied with success to five amputations of the leg and to one of the forearm.

ART. 93.—*On painless Cauterization.* By M. PIEDAGNEL.

(*Bull. Gén. de Thér.*, April 15, 1858.)

According to M. Piedagnel, an eschar may be caused without pain by using as the caustic a mixture of three parts of Vienna powder (caustic potass and quicklime) with one part of hydrochlorate of morphia. A paste is formed by mixing these powders with a sufficient quantity of water, alcohol, or chloroform, or else the powders are made up into hard dry discs by means of gum. M. Piedagnel also tells us that the process of blistering may be deprived of its painfulness by adding morphia in the same proportions to the powdered cantharides. As yet, this plan does not appear to have been sufficiently tried.

(F) CONCERNING INSTRUMENTS.

ART. 94.—*On the use of metallic sutures and metallic ligatures in Surgical Wounds and Operations.* By Dr. J. Y. SIMPSON, Professor of Midwifery in the University of Edinburgh.

(*Medical Times and Gazette*, June 5, 1858.)

In this paper Dr. Simpson gives some valuable historical and critical remarks respecting the use of metallic sutures in surgery. He also suggests that the use of metallic ligatures to secure and tie the blood-vessels laid open in the sides and depths of wounds made in the course of surgical operations and injuries, is a matter of as great, if not greater, moment than the use of metallic sutures to close the outer lips of such wounds.

The idea itself of employing metallic threads for surgical sutures is not entirely modern, however much the practice may be deemed so. In his learned dissertation on the "*Acia*" of Celsus, John Rhodius alludes to many different forms of thread, as the *filum*, "*lineum, laneum, sericum, xylinum, aureum, argenteum, ferreum, plumbeum.*" After speaking of the employment of gold and iron threads in the industrial arts, he alludes to the question of these two metallic threads being capable of use in surgical sutures; and evidently without ever having tried them, he condemns them as unfit for such a purpose. "*Alterutrum certè subtile*

admodum continendis vulnerum oris sine evidenti doloris molestia vi conferre potuit." (p. 192).

During last century, however, metallic sutures appear to have been used, in some isolated examples, by one or two surgeons. Thus, Purmann, "Chief Chirurgeon to the City of Breslau, in Germany," as he is styled on the title page of the English edition of his '*Chirurgia Curiosa*,' used, with alleged great advantage, metallic sutures in wounds of the tongue. The metallic sutures which he employed consisted of what has been specially recommended in modern times, viz., silver threads or silver wire. Needles of gold and silver were long preferred by most surgeons in applying the twisted suture for the cure of hare-lip. In his '*Elements of Surgery*,' published in 1746, Mr. Mihles speaks of employing silver and gold threads in the operation for hare-lip instead of pins, and figures a needle fitted to draw these metallic threads through the sides of the cleft lip.

The first surgeon in our own times who appears to have actually used metallic threads in practice, was the late Professor Dieffenbach, of Berlin. In a paper on '*Staphyloraphy*' published in 1826, he has detailed several instances of that operation, in which he used leaden thread to unite and keep united the sides of the divided palate. He preferred for this purpose threads of lead to threads of silk, as he found the ends of the leaden thread could be made by mere twisting of their elongated extremities, to bring into contact the raw sides of the wound more easily than could be effected by attempting to tie and knot the ends of silk threads, by introducing the fingers so deeply within the cavity of the mouth. "The difficulty," says Professor Fergusson, "of keeping the first noose steady has often been alluded to; the lead ligatures, by being twisted together, obviate this difficulty." The metallic suture in staphyloraphy has been alluded to by many later surgical writers (and modified by some), as, for instance, by Mr. Liston in 1831, Velpeau, Pancoast, &c.

Metallic sutures have been adopted in other plastic operations besides that of staphyloraphy. Gosset stitched together the sides of a vesico-vaginal fistula with gold wire, the gold threads being left in for twenty-one days. In his '*Practical Essays on Plastic Surgery*,' Mr. Spencer Wells observes: "The *lead* suture is sometimes useful in deep operations. A piece of soft lead wire is armed at both ends with a short needle. These are passed, by means of forceps or a needle-holder, from within outwards, and the needles removed. The ends of the lead wire are twisted together until the wound is brought into apposition. They are then cut off. This is the easiest suture to apply in cases of vesico-vaginal fistula when deep-seated. The only objection to its use is the necessity for protecting surrounding parts from irritation caused by the ends of the wire."

The use of metallic threads has been extended by some European surgeons to the stitching of common surgical wounds. In the '*British and Foreign Medical Review*' for April, 1846, p. 286, it is stated that platinum wire as a suture-thread has thus been "successfully employed at Guy's Hospital by Mr. Morgan." One of Mr. Morgan's colleagues at that hospital, the late Mr. Bransby Cooper, in his '*Lectures on Surgery*,' published in 1851, when speaking of the treatment of common surgical wounds by the interrupted suture, observes that this, "the in

interrupted suture, is the one more frequently used by surgeons, and silk is the ligature generally used; but platinum wire is preferred by some surgeons. As, however, it is rarely necessary or right to leave the sutures longer in the wound than forty-eight hours, I think (Mr. Cooper adds) it signifies little whether platinum wire or silk be employed." Again, Mr. Guthrie, when describing the treatment of wounds left by amputation, directs that "the common integuments of the stump should be drawn together in primary amputations by sutures formed of flexible leaden wire; by threads of silk, if leaden wire are not obtainable."

But in America the subject of metallic ligatures has met with more attention than in Europe. In 1832, Dr. J. P. Mettauer, of Virginia, employed them with perfect success in operating in a very aggravated case of laceration of the perinæum and rectum, produced, the year previously, by a tedious labour. The laceration extended as high as three inches upwards, along the anterior wall of the rectum. After sufficiently removing and denuding the hardened edges of the lacerated cleft, and the parts exterior to them, Dr. Mettauer stitched carefully together the abraded surfaces with ligatures of lead wire. "As the ligatures were applied they were tightened, so as to bring the abraded sutures in contact; and then their ends were twisted together, and cut off of convenient length. About twelve ligatures were required to close the wound. From time to time the ligatures were tightened by twisting them, and the vaginal margins of the laceration cauterized with nitrate of silver to favour the formation of granulations, which it was judged would greatly strengthen the union in this part" (p. 114). The bowels were constipated for four days. The leaden suture threads were not removed till six weeks, "the parts having united perfectly." In concluding his account, Dr. Mettauer observes, "leaden ligatures were preferred in the management of the foregoing case, as experience had proven them, not only less irritating and liable to cut out when tightly drawn than any other material with which I am acquainted, but infinitely more convenient and effective in maintaining a uniform and perfect apposition by the ready facility of simply twisting them, and a proof that the leaden ligature may act forcibly for a long time without cutting out. When they were removed in the present instance, it could not be perceived that any material encroachment had been made upon the margins of the cleft" (p. 115).

Four years after recording his first case in the 'American Journal of Medical Science,' Dr. Mettauer reported six additional instances in which he had operated for extensive lacerations of the perinæum. "In all of those cases," he states, "the recto-vaginal wall was completely divided, so as to convert the two passages bounded by it into one." Six of the operations were attended with complete success. In the seventh case, the wound partially tore open some weeks subsequently under the distension produced by the passage of "a large indurated mass of fæces, causing intense suffering;" and the patient had not yet submitted to a second operation for her cure. In all the cases in which he operated, Dr. Mettauer used the leaden thread as a suture; cutting it out, however, earlier than in his first case, or apparently from eight to twelve days after its insertion; and leaving the extremities of the wire

longer than at first, in order that they might be more readily seized and tightened by an additional twist or two, if they offered to become loose during the first few days following the operation.

In concluding this contribution, Dr. Mettauer observes, "My experience leads me to believe that every case of the afflictive accident is completely remediable. I decidedly prefer the metallic suture in the treatment of this infirmity. With it we are enabled to close and confine the denuded margin of the fissure with more ease and certainty than with the silken or thread suture. And should the least gaping of the wound take place, a few twists of the free ends of the wires will enable us to close it up again. The leaden suture, too, does not cut out as soon as silk or thread."

In the same year (1847) in which he published this second essay, on the cure of lacerated perinæum with metallic sutures, Dr. Mettauer published an account of some cases of vesico-vaginal fistula which he had treated on similar principles. In his first case the opening in the back wall of the bladder was "fully the size of a Spanish milled dollar, and nearly circular." Its edges were denuded and brought together with eight leaden sutures; and after the extremities of these sutures were twisted and tightened, the opening was perfectly close in every part of it, and the line of contact of the opposing surfaces measured two inches. A short, light, silver catheter was permanently retained in the bladder. On the third day the wires were tightened, and again on the seventh. On the thirteenth day the ligatures were removed, and perfect union was found to have taken place along the whole line of contact. The cure was complete, and the woman bore two children subsequently without any return of the accident. Dr. Mettauer operated in five other cases of vesico-vaginal fistula, but not always with the same success. In his second case the fistulous opening was diminished, but not obliterated, after eight operations. In two of the six cases Dr. Mettauer employed thread sutures, but he did not "find them to answer so well as the metallic." His results, however, on the whole, were so favorable as to induce him to conclude with the strong allegation, "I am decidedly of opinion that *every* case of vesico-vaginal fistula can be cured, and my success justifies the statement."

Dr. Marion Sims, formerly of Montgomery, Alabama, now of New York, published in 1852 an essay on 'The Treatment of Vesico-Vaginal Fistula,' describing his mode of operating, and his specialties of management in this class of affections. In this essay Dr. Sims, among other suggestions, recommended the lips of the fistula, after they were refreshed by the surgeon's knife, to be held together by threads of silver wire used as a suture. Latterly a "Woman's Hospital" has been established in New York, principally for the treatment of fistulæ and other injuries resulting from parturition; and Dr. Sims has, as surgeon to that institution, had ample means of proving the valuable and happy results of his treatment. His great and acknowledged success in the cure of urino-vaginal fistulæ, and their allied lesions, he himself attributes principally and essentially to the employment of sutures of slender silver wire instead of sutures of silk, &c. At the last anniversary meeting of the New York Academy of Medicine Dr. Sims read, and has lately

published, a discourse upon the use of "silver sutures,"* displaying (to adopt his own words) "all the ardour and enthusiasm of a devotee." In this discourse he proposes to extend—and relates, indeed, various cases, showing that he had in his own practice extended—the use of silver sutures from vesico-vaginal fistulæ to all the common wounds and operations of surgery. Speaking of silver wire as a suture, he remarks, "From the day its wonderful effects were witnessed in vesico-vaginal fistulæ in 1849, I have never used any other suture in any department of surgery" (p. 32); and "I declare it (he elsewhere observes) as my honest and heartfelt conviction that the use of silver as a suture is the great surgical achievement of the nineteenth century" (p. 8).

Dr. Sims further indulges in the following enthusiastic and prophetic remarks regarding the value of the silver suture and its "universal applicability in general surgery:" "It is (he says) to revolutionise surgical dressings, and to ensure more beautiful and prompt cures. With it, properly applied, there can be no gaping wounds to heal by the suppurating process, where there is skin enough to cover a stump; and in many cases erysipelatous inflammation, and even hospital gangrene, may be averted by substituting it for silk as a suture. After all amputations we must use sutures of some sort; and how often do we see silk ulcerating out, and creating such tendency to suppuration, that we are compelled to remove them before there is sufficient union to resist the retraction of the tumefied flaps. But with silver there is no inflammation, no suppuration, no cutting out of sutures, no gaping or retraction of flaps, and therefore no necessity for disturbing the dressing till all is firmly united and permanently well. This," Dr. Sims adds, "is no vain imagining; though enthusiastic, I am not wildly so, for all this has been familiar to me for the last eight years, and I but speak what I know. The next eight years will not find an educated physician anywhere who will dare to use silk sutures, for the silver-thread will now become as essential to the dressing-case as the needle itself; and if I may be allowed to venture a prediction, I will say that fifty years hence the statistics of our hospitals will show a vast improvement in their bills of mortality after great operations, and this improvement will be due mainly to the use of silver as a suture. Look at its results in injuries of the vagina. Before this discovery, operations for vesico-vaginal fistula, and its congeneric affections, were often attended with risk to life, while a cure was a mere accident. But how is it now? Why, every case is easily and perfectly curable that has tissue enough to render any operation whatever practicable; while a failure is the exception to the rule. Besides, there is not the least risk to life, as there is never any fever, or the slightest constitutional disturbance. I am not claiming too much for this suture when I say, that the same relative results must be attained in all other surgical operations requiring sutures, if the same method be adopted. My language is nowise extravagant; and I shall yet live to see the day, when the whole profession of the civilised world will accord to this simple discovery the high position of being the most

* 'Silver Sutures in Surgery;' the Anniversary Discourse before the New York Academy of Medicine. By J. Marion Sims, M.D. New York, 1858.

important contribution as yet made to the surgery of the present century," (pp. 44 to 46).

The very earnest and unusual terms in which Dr. Sims thus describes the advantages of silver sutures, indicates at least a profound and intense conviction on his part of their great and unqualified superiority over sutures of silk and common thread.

ART. 95.—*Silver Sutures in Surgery.*

By Dr. J. MARION SIMS, of New York.

(*Anniv. Discourse before the New York Acad. of Med.* Pamphlet, 1858.)

Dr. Marion Sims is a most enthusiastic advocate for the use of silver sutures in surgery, and this discourse is intended to show that he discovered them, and that a new era has dawned upon surgery in consequence. For other particulars we beg to refer to the end of the preceding article, where Dr. Simpson has said all that need be said upon the subject, in a way which we cannot improve upon.

ART. 96.—*A new Tourniquet.* By Dr. S. D. GROSS.

(*North American Medico-Chir. Rev.*, Jan., 1857.)

This instrument is a forceps-shaped compressor, resembling somewhat a pair of double callipers, opening on a central screw-pivot, with long blades of large curvature at one end and short blades of small curvature at the other, both pairs of blades being regulated by the same ratchet. A double tourniquet or a pair of tourniquets is thus produced—a large one for the thigh and a small one for the arm or for the thigh of a small person. The upper and lower blades on each side of the central screw differ also in length and curvature. The upper blades are shorter, narrower, and of a smaller curve; their extremities, being each provided with a pad which works upon a screw, dip downward so as to direct the pad upon the artery, and by means of a few turns of the screw of the pad, to press the latter firmly on the vessel. The lower blades are longer, broader, and more widely curved, being intended to maintain the counter-pressure on the outer side of the limb which the instrument is intended to grasp, while avoiding circular constriction, as in the tourniquet of Dorsey and the later instruments of Signoroni, Skey, and others.

For a better comprehension of the instrument we must refer to the wood-cut illustration which accompanies the inventor's description.

The advantages claimed for it are—first, facility of application; secondly, the amount of pressure commanded; thirdly, its ready adaptation to limbs of different sizes; fourthly, its confining the pressure and counter-pressure to the two opposite surfaces where alone it is wanted; and lastly, the ease with which it may be slackened or removed at any stage of the operation. With a slight modification it might readily be adapted to the femoral artery under Poupart's ligament, or to the external iliac just above, or to the axillary artery.

(G) CONCERNING ANÆSTHETICS.

ART. 97.—*Acetone as a new Anæsthetic.* By M. BECHAMP.

(Rév. Thér. des Méd., June, 1858; and Journ. de Physiologie, July, 1858.)

M. Bechamp appears to have made several experiments with this substance, and with very satisfactory results. Acetone, he tells us, is less disagreeable to inhale than amylene, and at the same time more rapid in its action and less permanent in its effects. It acts upon rabbits in thirty seconds, and the insensibility is complete. He tells us also that these rabbits recovered after a very prolonged inhalation, and his inference from this fact is that it is less dangerous than chloroform or amylene.

ART. 89.—*On the inhalation of Carbonic Acid as a safe and efficacious Anæsthetic.* By M. OZANAM.

(Archiv. Gén. de Méd., April, 1858.)

The inhalation of this gas, M. Ozanam tells us, produces effects which are very analogous to those of ether, only more transitory; and these effects he considers as belonging to four periods.

1. *The prodromic period.*—In this period the animal experimented upon is sometimes calm, sometimes rigid. The duration is from one to four minutes, according to the strength of the subject and the dilution of the gas with atmospheric air.

2. *The period of excitement.*—This is almost absent, and at most it consists in some agitation and voluntary movement. The respiration is quickened and so are the beats of the heart, then, in about a minute, the muscles relax.

3. *The period of Anæsthesia.*—In this period the animal lies stretched on its side, breathing slowly and profoundly, and with the pupil moderately dilated, and with the heart beating slowly and more feebly. The anæsthesia is complete. M. Ozanam tells us that this state of complete anæsthesia may be kept up by continuing the inhalation, without any danger to life, for ten, twenty, thirty minutes, or more, and that the animal recovers almost immediately when it is allowed to breathe atmospheric air.

4. *The period of waking,* which is very transitory, appears to be somewhat like a state of drunkenness.

One curious fact transpires in connexion with these experiments, and this is, that animals which have been frequently submitted to them, at length become so habituated to the gas, that they cannot be rendered anæsthetic by it.

ART. 99.—*On local Anæsthesia and Electricity.* By Dr. RICHARDSON, Physician to the Royal Infirmary for Diseases of the Chest, &c.

(*Medical Times and Gazette*, Sept. 11, 1858.)

Dr. Richardson's attention was directed to this subject in 1813. He says :

"I was then engaged in investigating by experiment the influence of electricity on the blood in the living animal body. In one of these experiments a small dog was subjected to an electrical shock, resulting from the discharge of a battery of seventy-two Leyden jars. Wire chains ready for connexion with the battery were placed—one round the throat of the animal, meeting over the upper part of the head, the other round the lower part of the body at the loins. The whole charge was at once passed through the body. The animal fell without a struggle, and lay before me to external appearance dead. There was no respiration for several seconds, but the heart continued beating. A little later, and there was a feeble respiratory gasp. I pricked the nose of the animal with the point of a scalpel, and blood issued, but no indication of sensibility on the part of the animal followed. A minute more, and I had laid bare about an inch of the right jugular vein. I tapped the vein, drew off a few drachms of blood for after-observation, passed a ligature round the vessel above the opening, brought the edges of the flesh-wound neatly together, and secured them by suture. By the time I had done, the signs of reanimation were well marked, but the operation had been performed without the slightest evidence of suffering. For a little time the respiration was short and irregular, but in a few minutes the animal rose slowly, looked about him, as if wondering where he had been, and recovered without a bad symptom.

"This was probably the first instance in which any operation was performed without pain, by means of electricity. This result of the experiment was purely accidental. The experiment was originally intended for a different object altogether; but, accustomed to operate on narcotized animals, the new fact of the perfect production of insensibility by electricity changed the intention of the experiment entirely in my mind. The idea of producing general anæsthesia for the purpose of an operation, by a repetition of this one electrical experiment, was of necessity out of the argument, for it were impossible so to adjust a shock as to produce a sufficient degree of general insensibility for an operation without the hazard of destroying life altogether. The fact of the production of insensibility was, however, striking; and this fact at once suggested to me that what could be done to the whole body might possibly be done to a part.

"To carry out the inquiry which had thus been presented, I tried the effect of passing electrical shocks of varying intensities through the limbs of animals. The shocks were severely felt in these cases, but I could never detect that at any instant after the shock the sensibility of the parts through which it had passed was at all destroyed. There was often some temporary twitching of the muscles of the limb operated on, but the merest attempt to produce pain succeeded.

"I next tried various experiments on myself. I charged twenty

Leyden jars, and discharged them, either in combinations or one after the other in rapid succession, through one of my fingers. The shocks were painful to bear, and, when many were given, the last was felt as severely as the first; but afterwards the finger was as sensitive to a prick from the point of a lancet as it had been previously.

"I tried the local effect of the continuous current for long periods, but with as little success.

"I passed the electro-magnetic current through one finger for long periods, modifying the intensity of the shocks; sometimes submitting the part for periods of an hour or more to a gentle current; at other times increasing the force till the pain produced was scarcely endurable. On January 3d, and on August 8th, of the present year, I kept a finger for two hours thus exposed; but in these, as in all other cases, without the slightest effect in removing sensibility. While the finger was being subjected to the current, I tested its sensibility by pricking it with a lancet or needle. This test is, however, unnecessary, for so long as the part operated on is sensible of shock, it is sensible to a cut or a puncture. An animal deeply narcotized with chloroform is as little sensitive to electrical shocks as it is to the knife.

"In its local application, indeed, it seems to me that the electric current restores rather than destroys sensibility. One experiment will explain this. Let two fingers be placed in a freezing mixture, and held there until the external surface is so benumbed that the prick of a needle is not felt. Let them then be removed, and let pass through one the current from the electro-magnetic battery. In the finger thus operated upon, it will be found not simply that the sensibility will come back more quickly, but so much the more quickly as to lead to the unpleasant and painful reaction called vulgarly 'hotache.' The current acts like warmth in this respect.

"In one experiment the effects produced were very peculiar, and deserve special note. I placed the first and second fingers of the left hand in a mixture of ice and salt till they were entirely insensible to puncture. I then removed them from the mixture, and, after well drying them, I placed one wire from the electro-magnetic battery round the second finger, at a distance of three-quarters of an inch from the tip, and the other wire round the finger at the base. A gentle current was passed. For a brief period I was not conscious of the shocks, but suddenly the portion of finger included between the wires from being white became red and injected, and therewith there was excited a degree of pain that was unendurable. By removing the wires, and applying cold once more, the acute pain passed off. But the most interesting point is, that while the first finger regained its normal sensibility in the course of an hour, and the second regained its normal sensibility in the parts which had been inclosed between the wires, the end of this second finger, from the point beyond which the upper wire had encircled it, remained completely insensible for four hours, and felt slightly numbed even thirty-six hours later.

"From these experiments I have no alternative but to believe that the electric current cannot, according to our present knowledge of its application, be made practicable for the production of local anæsthesia.

"The only way by which, as I would suggest, electric shocks can in

any way be said to remove pain locally, is, that the pain which they excite creates a diversion, so that any new pain which may be inflicted on the part is not felt the less, but is lost in some degree in the pain which was preexistent. I give a simple illustration. The schoolboy tells his new comrade that he can remove a hair from his head without the removal being felt. The skilful operator seizes a hair with his left thumb and finger, pulls it out quickly, and at the very moment strikes the head of his dupe a smart blow with his flat right hand. The operation is performed, and it may be without the pain which would have been elicited by a simple pull. The pain, however, is not removed, but diverted. When my finger was painfully affected by the electric current, the entrance of the lancet or needle-point into the skin caused sometimes a more acute, sometimes a less defined, pain than is ordinary. Mr. Louis Parnell, who allowed me to perform some experiments on his finger, expressed that his sensations were the same.

"We have seen, nevertheless, in the first experiment related, that a powerful electric shock, sent through the whole body, will produce insensibility; why, therefore, should it not have the same effect in its local application? A dose of aconite tincture will render a body generally insensible to pain; a drop of the same tincture, put upon the lip, will produce numbness of the lip. Here is brought out at once a general and a local effect, each alike in kind, but different in degree. Why, then, should not the same obtain with the electric shock? To answer this, the consideration of the modes in which insensibility is ordinarily produced is necessary."

ART 100.—*Electricity as an Anæsthetic in Toothdrawing.*

By Mr. FRANCIS, of Philadelphia, and others.

(*British Med. Journal*, Sept. 7, 1858.)

Mr. J. B. Francis has recently devised a new plan for producing insensibility during the extraction of teeth. In the 'American Journal of Dental Science' for July, 1858, we are informed that a number of operations of this kind had been rendered painless by means of electricity, in the presence of the Sub-Committee on Science and Arts constituted by the Franklin Institute of the State of Pennsylvania, and of other gentlemen. The exclusive right to the application of electricity for the producing local anæsthesia in the extraction of teeth has been secured by Mr. Francis, who thus has, for a term of fourteen years, the sole privilege of "making, constructing, using, and vending to others to be used," the necessary apparatus. The news of this discovery, on reaching this country, has excited much interest. A considerable amount of correspondence on the subject has recently appeared in the *Times*, from which we make such extracts as will show the *modus operandi* of the agent.

Mr. W. K. Bridgman, of Norwich, describes the apparatus required, and its application, as follows:

The apparatus is extremely simple, and consists principally of the common electro-magnetic machine used in medical electricity, a single cell and pair of plates constituting a Smee's battery, and a small electro-

magnetic coil, with a bundle of wires for graduating the strength of the current. One end of the thin wire conveying the secondary current is attached to the handle of the forceps, and the other end of it to a metallic handle to be placed in the hand of the patient. The instrument touching the tooth completes the circuit, and the current passes instantaneously. The wire attached to the forceps should be made to pass through an interrupting footboard, so that the continuity of the wire may be made or broken in an instant by a movement of the right foot of the operator. The advantage of this arrangement is, that it allows the instrument to be placed in the mouth without risk of producing a shock in coming in contact with the lips, cheek, or the tongue. A hole drilled in the end of the left handle of the forceps, and the end of the wire tapered to fit rather tightly, allows the substitution of one pair of forceps for another with scarcely a moment's delay.

Some cautions, however, are necessary. Mr. Joseph Snape, dentist to the Chester Infirmary, says:

"The current should not be applied to a tooth with an abscess at the root, to a loose tooth, or to fangs imbedded in spongy gums, as the pain, in such cases, appears to be increased. When the sides of a tooth are decayed nearly or quite to the gum, the tooth and surrounding parts should be rendered as dry as possible, by means of French bibulous paper. Fangs, when the gums are not spongy, may be extracted with the forceps after being treated in the same manner."

Again, Mr. C. Spence Bate, in the same paper, says:

"The application of the wires from an electric coil and Smee's battery, higher charged than $1\frac{1}{2}$ deg., is in itself very painful to the gums and internal mouth. It is therefore an object of much nicety to regulate the amount of electricity, so as to preclude a painless operation becoming a very painful one, either from too much or too little electricity being administered. In his own case, Mr. Bate felt a bearable amount of electricity when the instrument was placed upon the tooth (under wisdom-tooth), but the same amount applied to the gums over the front teeth was excessively painful. The quantity of electricity that made the operation painless to him was not enough to kill the sensation of pain in a gentleman, for a similar tooth; whereas, in a lady, shortly after, it was quite successful with a less amount. In placing the instrument in the mouth, it is desirable that the broken contact in the negative rod should be restored before the instrument comes into contact with the gum, since the insertion of the former beneath the latter is one of the most painful parts of the operation in tooth-extraction.

II. SPECIAL QUESTIONS IN SURGERY.

(A) CONCERNING THE HEAD AND NECK.

ART. 101.—*Thirteen cases of Recovery after fractured base of the Skull.*
By Mr. PRESCOTT HEWETT, Surgeon to St. George's Hospital.

(*Medical Times and Gazette*, June 12, 1858.)

Mr. Prescott Hewett has been able to gather together some thirteen cases of recovery after a fractured base of the skull, in which the state of the broken bones was ascertained by dissection some time after the accident.

CASE 1.—A man met with a severe injury of the head, which subsequently led to an affection of the brain, and he died three months after the accident. An extensive fracture, which had not been suspected, was then discovered at the base of the skull; this fracture beginning in front of the mastoid portion, ran through the extremities of both petrous bones, and through the sella Tursica. The broken bones were about a line distant from each other. No attempt had been made at reparation.

CASE 2.—In a case reported by Mauran, the patient survived an injury of the head three years, during which he suffered greatly. When the head was examined, a piece of the petrous bone was found broken off, and around this an abscess had formed.

CASE 3.—In a case reported by Mr. John Davies, of Hertford, the patient committed suicide five months after a severe injury of the head, the symptoms of which had been those of a fractured base. In the left temporal bone were the remains of a fracture running obliquely upwards and outwards between the squamous and petrous portions, splitting the Eustachian canal, as far as the tympanum, and another line of fracture also extended from the tympanum to the groove of the lateral sinus. The line of fracture was so imperfectly united that the broken parts of the bone were separated from one another during the working of the saw.

CASE 4.—In a case, which was under the care of Mr. Stanley, in St. Bartholomew's Hospital, in 1840, the patient, æt. 9 years, died on the eighty-sixth day after a severe injury of the head, accompanied by bleeding from the ear. A line of fracture was traced from the squamo-parietal suture to the petrous portion, through the middle fossa of the skull. The bones were quite separated, and without the slightest traces of an attempt at union. The edges of the bones were bloody, and not in the least rounded off. And what makes this case still more interesting is that the clavicle, which had also been broken at the time of the accident, was firmly united by callus, ensheathing the broken extremities of the bones, and completely fixing them.

CASE 5.—A healthy man, 40 years old, fell from a height of twelve metres on to his feet. The only immediate symptoms were those of slight concussion, which soon passed off. But the accident was subsequently followed by cerebral mischief, which led to the death of the patient four months afterwards. Latterly, the case was under the care of M. Robert, in the hospital Beaujon. At the examination of the head, the posterior clinoid processes

were found broken off from each other, and from the lamina quadrata; the right petrous bone also presented in its upper third an extensive fracture running in a transverse direction; a large fragment was completely separated from the rest of this bone.

CASE 6.—A man, æt. 45, was admitted into the Glasgow Infirmary, for a severe injury of the head, with bleeding from the right ear, caused by a fall down stairs, which had happened six days before. Ultimately, this patient so far recovered as to be dismissed from the infirmary at the end of eight weeks. Nine days afterwards he was seized with cholera, and died on the 5th day, under the care of Dr Laurie. He had thus lived close upon eleven weeks from the time of the accident. On removing the dura mater, and carefully examining the bone, a fracture was found to extend from the superior part of the right parietal bone along its posterior margin, and a few lines from the lambdoid suture, joining the suture between the occipital and temporal bones, and terminating at the jugular fossa. On the interior of the right half of the occipital, and a portion of the corresponding parietal bone, there was a considerable deposit of ossific matter which had nearly obliterated the groove of the lateral sinus. Between the fractured surfaces of the bones, there were a few particles of ossific matter deposited, but the junction was far from being completed.

CASE 7.—M. Richet states that he had under his care a man who had met with an injury of the head, which was followed by a serous discharge from the ear. Recovery took place, for a time at least. But five months afterwards, and without any premonitory symptoms, strabismus made its appearance; this was followed by cerebral symptoms, and the patient died. At the post-mortem examination a fracture was discovered in the malar bone, from whence it was traced into the base of the skull, cutting across the petrous bone and terminating in the foramen magnum. At first no trace of the fracture could be discovered in the petrous bone, notwithstanding the most careful examination; and it was not until after this bone had been macerated in nitric acid for a month, that the course of the fracture was clearly made out.

The other six cases are made up of preparations in different metropolitan hospitals.

CASE 8.—In the first of these preparations* the line of fracture, starting from the outer part of the right frontal eminence, passes downwards, behind the external angular process, into the great wing of the sphenoid, and obliquely across the back part of the orbiter plate of the frontal, then through the lesser wing of the sphenoid into the body of this bone, where it terminates at the inner part of the left optic foramen. This extensive line of fracture is, in some parts, completely filled up by new bone inlaid between the edges, and in other parts there was merely a dense layer of fibrous tissue lying between the broken bones. The most perfect union is in the perpendicular portion of the frontal, and especially in the lesser wing of the sphenoid, where the fracture is scarcely perceptible. The inner surface of the bones presents, in some parts, an extensive deposit of new, spongy bone, extending, in several places, far beyond the line of injury. The patient, a middle-aged man, lived two months after a very severe injury of the head, from the effects of which he had, however, all but recovered, when he was attacked with erysipelas of the scalp, and died.

CASE 9.—The second preparation† was taken from a man, æt. 45, who died of malignant disease of the stomach and liver. Some nine weeks before his death the man had met with a severe injury on the left side of the head, ac-

* St. George's Museum, ser. i, sub-se. xxvi, A 1 and 2.

† St. George's Museum, ser. i, sub-se. . . . A 5.

accompanied by bleeding from the left ear, and from the nose. The preparation shows a fissure, from three to four inches long, passing from the central part of the left parietal obliquely through the squamous portion of the temporal, where it divides into two branches, one running onwards, as Mr. Gregory Forbes reports, to the centre of the petrous bone, and the other passing into the meatus externus. The edges of the main line of fracture are, in one part, widely separated, extensive absorption of bone having taken place, and the gap thus left is one inch and a half long, and the eighth of an inch wide at its broadest part. Mr. Tomes examined this preparation in its recent state, and found that there was no bony union even at the points, where the edges of the fracture were close to each other. In the second line of fracture, that passing into the meatus, the edges are in close contact, and apparently united by inlaid bone, the line of injury in this part being scarcely perceptible. The inner surface of the bones is, in some parts, roughened by the deposit of new bone in the neighbourhood of the lines of fracture. The case has been fully reported by Mr. Gregory Forbes,* who presented the preparation to the museum of St. George's Hospital.

CASE 10.—The third preparation† was taken from a man, æt. 46, who died in St. George's Hospital, of extensive ulceration of the stomach. Three years before his death this man, it appeared, had been pitched from his horse on to the back of his head, the results of which were a severe scalp-wound, and delirium for several days after the accident. In this preparation a distinct line of fracture can be traced through the left side of the occipital bone, from the upper part down to the point where the groove of the lateral sinus terminates in the jugular foramen. In its upper part this extensive line of fracture is closely united by inlaid bone; externally, there is merely a linear groove, and internally the union is even more perfect, the line of fracture being visible here and there only. All this part of the bone is very much thickened, and extensively perforated by minute holes, some distance even beyond the line of injury. In the lower half of the fracture the edges of the bone are thin, and bevelled off by absorption; here there is a slight gap, which, in the recent state, was filled up by dense, fibrous tissue. At the end of this fissure, just where the groove for the lateral sinus terminates in the jugular foramen, the bone looks as if it had been broken up into several pieces. The union is perfect, but the lateral sinus is at this spot all but obliterated, and the occipital is here ankylosed, on the one hand, to the petrous bone, at the jugular articulation, and, on the other hand, to the atlas, at the condyloid articulation.

CASE 11.—The fourth preparation‡ was presented to the College Museum by Mr. Henry Lee. It was taken from a man, æt. 45, who lived a little more than seven months after a severe injury of the head, with bleeding from the ears, nose, and mouth. He was killed on the spot by a second injury, involving the upper part of the spine. The preparation shows a line of fracture, passing nearly straight downwards through the squamous portion of the right temporal bone, to the upper margin of the meatus auditorius externus; thence it proceeds along the upper wall of this passage, a part of the anterior wall being destroyed; and opposite the cavity of the tympanum it branches in two directions; by one of these branches, the fracture extends through the posterior and lower border of the petrous portion of the bone into the jugular fossa, which it completely traverses; by the other, it extends along the upper surface of the petrous bone, and ends in the hiatus Fallopii.

* *Lancet*, 1849, vol. i, p. 580.

† St. George's Museum, ser. i, sub-sc. xxvi, A 3.

‡ College Museum, series xii, sub-series iii, section B, 483.

That part of the fracture which lies in the squamous portion of the bone is, with the exception of a few minute apertures, closely and smoothly united; but in the part which traverses the petrous bone, there are only two or three small points at which union has taken place. At the upper surface of the base of the petrous bone, there is a considerable loss of substance, leaving a large hole leading directly into the cavity of the tympanum. The margins of this hole, and the bone around, are smooth and rounded off, as if from absorption.

CASE 12.—The fifth* preparation belongs also to the College Museum. There is no history of the case; but this skull, it is thought, was removed with several others from the burial-place of some establishment for invalid soldiers in Germany. The occipital shows, on its left side, an extensive fracture, of an oval shape, passing from the upper part of the bone to within a short distance of the foramen magnum. At the lower part, there is a considerable displacement of the fragments, some of which have slipped downwards, and overlap a large portion of the outer surface of the bone. The displacement of the fragments has left, at the upper part, two irregular openings, the margins of which are smooth and bevelled off. The fragments are all solidly united to each other, as well as to the surface of the bone with which they have been brought into contact.

CASE 13.—The sixth preparation† is in the Museum of St. Bartholomew's Hospital. The history of the case is unknown. The squamous portion of the temporal bone was smashed, and a line of fracture extended through the meatus externus, and base of the petrous bone, nearly as far as to the edge of the foramen magnum. The fragments in the one part, and the line of fracture in the other, are united firmly, but with intervals of non-union.

ART. 102.—*A new mode of treating purulent Ophthalmia.*
By M. DE CONDÉ.

(*Annales d'Oculistique*, t. xi; and *Medico-Chir. Rev.*, Oct., 1858.)

M. De Condé, a Belgian military surgeon, dissatisfied with the present treatment of this disease, which seems to be very prevalent still in the Belgian army, lays down a new plan, based upon the following considerations:

1. The greatest danger to the eye arises from the contact of the upper lid, which, inflamed and swollen, floods its surface with an acrid and corrosive pus. The excessive heat of the eyelid, the internal surface of which is rough and unequal, interferes with the nutrition of the cornea, while the acidity of the pus leads to its softening and destruction.
2. It is admitted that it is of importance to prevent the contact of inflamed mucous membranes by the interposition of an isolating body, such as charpie or wadding. This is seen in vaginitis, balanitis, and fissure of the anus.
3. This body, which may alone produce great amelioration, or even in some cases a removal of the disease, may exert a powerful effect if impregnated with an active agent. In this way lint, soaked in a concentrated solution of acetate of lead, and placed between the glans and the prepuce, will cure gonorrheal balano-posthitis within forty-eight hours.
4. Cod-liver oil exercises a powerful action in disease of the mucous membranes, modifying and then suppressing their secre-

* College Museum, prep. 2899, A.

† St. Bartholomew's Museum, series iii, c. 94.

tions. It strengthens the fibrous tissues of the eye and the cornea, and tends to prevent ramollissement. It is especially in ulceration and chronic ramollissement of this membrane that this double action is perceived. 5. The red precipitate ointment (four parts to fifteen of lard and fifteen of linseed oil) is an excellent substitutive agent, sufficing alone to arrest the disease when employed early. It is the best remedy for cutting short the ophthalmia of new-born infants. 6. A solution of the chloride of lime (thirty parts to two hundred of water) is an energetic modifier, neutralizing with certainty the virulence of the secretions. 7. Lastly, perchloride of iron exerts an instantaneous hæmostatic effect upon the hæmorrhagic mucous membrane, and an indubitable modifying influence upon the mucous secretion.

As an isolating body, M. De Condé, after trying various substances, gives the preference to wadding, as forming at once an inoffensive application, capable of imbibing and favoring the flow of the secretions, and of being impregnated with medicinal agents. A slip of the wadding somewhat longer than the transverse diameter of the eyelids, is brought opposite the palpebral aperture, and gently pushed up beneath the upper eyelids by means of the little finger or a large probe. Some cases are related as examples of the manner in which a disease often so tedious in its progress may, upon these principles, be rapidly cured.

ART. 103.—*On Dr. Von Græfe's operation for Glaucoma.*

By DR. VON GRÆFE.

(*Ophthalmic Hospital Reports*, No. 3, 1858.)

Dr. Von Græfe writes to correct certain errors and misstatements which, in his opinion, are to be met with in a recent paper by Mr. Critchett (*vide* 'Abstract,' XXVIII, p. 288).

"Mr. Critchett," he says, "assumes in glaucomatous disease an increase of the intra-ocular pressure, and explains the curative action of iridectomy by the reduction of this pressure. He opposes this explanation to one presumed to have been given by me, which is based upon the restoration of the communication between the anterior and posterior chambers. I cannot but suppose that he here confounded two completely different works of mine, one of which is entitled, 'On the Curative Action of Iridectomy in Chronic Iritis and Iridochoroiditis,'* and the other, 'On the Curative Action of Iridectomy in Glaucoma.'† The explanation imputed to me by Mr. Critchett I have indeed given for the cases of iritis and iridochoroiditis, in which total posterior synechia exists, or where the iris is driven forward by exudation lying behind it; but it has never occurred to me to bring this explanation to bear on glaucoma. It has been rather the essential point of all my publications on glaucoma to base the nature of the disease on the increase of intra-ocular pressure. I have, in order to find grounds for this assumption, not only made use of the known external symptoms of the disease, as Mr. Critchett has done, but I have also taken advantage of the ophthalmoscopic ones (excavation of the optic nerve and pulsation of the arteria

* See 'Archiv fur Ophthalmologie,' vol. ii, part 2.

† Ibid., vol. iii, part 2.

centralis retinæ), and not without arduous study. It was exactly this view which led me, as it has now Mr. Critchett, to the methodical carrying out of the remedies for diminishing pressure, specially in cases of glaucoma,* and not of internal ophthalmia in general. Finally, I arrived in this manner at iridectomy, because a number of experimental facts and pathological proofs argued that this operation is followed by a *permanent* reduction of the intra-ocular pressure. In fact, every attentive reader of my works must concede that the theory given now by Mr. Critchett agrees literally with mine, and there does not exist the slightest difference of opinion between us.

“The method employed by me, for the last two years, of operating on glaucoma, is by no means injurious to the eye, as Mr. Critchett believes. After two, four, or at the farthest, six days, I dismiss from the wards my patients, who are constantly under the observation of a number of confrères interested in the subject. Although I have operated on upwards of a hundred cases of glaucoma, in the strict sense of the word, I have never known the slightest casualty to occur. The statement that I excise a third of the iris is decidedly erroneous. This would be also impossible, as I perform the iridectomy quite in the usual way, by a linear incision. I certainly recommend a larger portion of the iris to be excised than when a simple artificial pupil in leucoma adhærens is concerned, as I prefer in the latter case, from optical reasons (in common with my English colleagues), a small pupil; whilst, in glaucoma, greater experience has taught me, that, particularly in older cases, the excision of a small piece of iris does not exercise so complete and permanent an effect on the intra-ocular pressure as the excision of a larger piece. Notwithstanding this, however, *I never excise more than one sixth of the iris*. The fact that I do not commence my incision in the cornea itself, but one millimetre behind it, in the sclerotic, can only diminish the injury to the eye. I never leave a portion of the iris in the wound, as does Mr. Critchett, because, according to my experience, the cure is thereby retarded. Sometimes a small prolapsus iridis is produced, but not the slightest advantage results from it. Even the advice to perform the excision in the upper part of the iris, when cosmetic considerations are to be taken into account, is found in my earlier work, ‘On Artificial Pupil in Iritis, &c.’; it is repeated in my later work on glaucoma, and has been practised by me for some years. Truly, I cannot distinguish the slightest real difference between Mr. Critchett’s method and my own, which would deserve to be mentioned among professional confrères in such a manner as Mr. Critchett has done. . . . I only regret here, as on many other occasions, that the wide space which separates men who devote themselves with similar zeal to the same studies, prevents them from communicating personally in an authentic form the results of their experience.”

* “With regard to the employment of mydriatics and paracentesis in glaucoma, I refer to my earlier publication in the ‘Archiv für Ophthalmologie,’ vol. i, part 2, page 303.”

ART. 104.—*On the abuse of Mercury in Ophthalmic Diseases.*

By Dr. ALFRED POLAND, Surgeon to Guy's Hospital.

(Lancet, May 15, 1858.)

The chief aim of the author is to draw the attention of the profession generally to the above subject, and more especially when practised by those not over-conversant with these affections. He is sorry to say that the study of eye-diseases has considerably declined, and that it is more and more becoming an isolated branch of the profession. In former years every hospital and general surgeon undertook the treatment of this class of cases, and the lectures on surgery at the several schools duly elucidated the principles and practice thereof. Nowadays, on the contrary, with the exception of one or two leading medical schools, ophthalmic surgery had become isolated, and rendered perfectly distinct and separate; there are separate wards and separate lectures, which are, moreover, unrecognised by the examining boards. The author hopes that this state of affairs might be remedied, and that persons before entering on practice should show some evidence of having studied, and become practically acquainted with the treatment of diseases of the eye. With these strictures the author cursorily surveys the general diseases of the eye, pointing out that in the majority of them mercury was not required. In the conjunctival inflammation, including also gonorrhoeal ophthalmia, mercury was inadmissible, inasmuch as they could be cured without its administration. So, also, in so-called strumous diseases, comprising ophthalmia and corneitis, it was a remedy that did no good, as attention to the secretions and excretions, with due regard to diet and the powers of the patient, sufficed to eradicate the complaint. It would hardly be credited that, in the year 1858, mercury was administered for penetrating wounds of the eye, yet he (Mr. Poland) was sorry to say such was the case; and, he remarked, what would be the results of operation for extraction were this line of treatment to be adopted? In adverting to scleritis and iritis, mercury came to our aid, more so in the latter than in the former disease; yet even in these affections more attention ought to be paid to the condition and constitution of the patient than the nature of the disease. It was too much the fashion to consider that because a part or organ had taken on inflammation, and, in consequence, had appended to its name the dreaded term "itis," it was to be combated by calomel, leeches, low diet, &c. Many an eye, and, perhaps, life, had been lost by this unfortunate appendage. As for amaurosis, the author naively stated that all the mercury in the world would not replace a cupped condition of the entrance of the optic nerve, nor would it remove a detached retina, nor promote the absorption of dirty white pigment on the retina, nor restore the lost transparency of the retina, nor such other organic changes pointed out by the use of the ophthalmoscope. Yet mercury was, and is still, administered by some, for days, weeks, and even months. Space will not allow us to enter more fully into each of the topics brought forward, as these principles and treatment advocated are those generally practised and taught by the several leading surgeons who have studied eye-diseases. It was the author's endeavour, in his

few loose remarks, to call attention to the simple fact, that an improvement had taken place in the last few years, by a more guarded abstinence from the use of the drug mercury in ophthalmic practice, and that such should be made known to the profession generally, so that they may avoid resorting to the remedy as a specific in inflammations generally.

ART. 105.—*On the different forms of inflammation of the Eye consequent upon inherited Syphilis.* By JONATHAN HUTCHINSON.

(*Ophthalmic Hospital Reports*, No. 4, 1858.)

Mr. Hutchinson's objects in this essay are :

I. To give a more detailed account than has yet been attempted of the form of acute iritis, occasionally met with in syphilitic infants.

II. To show that acute iritis, as a consequence of hereditary syphilis, is not so very rare as it has been thought, and that it now and then occurs at periods subsequent to infancy.

III. To endeavour to prove that the form of kerato-iritis met with in young persons, and formerly known as "aquo-capsulitis," is, in the majority of instances, of specific origin.

IV. To endeavour to prove that the disease known as "chronic corneitis," "interstitial corneitis," "strumous corneitis," is almost always a direct result of inherited syphilis.

V. To illustrate the connexion with hereditary taint of some of the cases of deposit in or upon the retina or choroid, hitherto classed as "scrofulous."

VI. To show that certain cases of tinea tarsi, fistula lachrymalis, and other affections of the ocular appendages occurring in children, are of syphilitic origin.

ART. 106. — *Why does the operation for Strabismus sometimes fail?*
By Mr. HOLTHOUSE, Surgeon to the Westminster and South London Ophthalmic Hospital, &c.

(*British Med. Journal*, July 17, and Sept. 4, 1858.)

Confirmed strabismus may be due either to a shortened or hypertrophied condition of one of the recti muscles; or this may be combined with contraction of the conjunctiva and the subconjunctival tissue, and an adhesion of these to the sclerotic coat of the eye: the cure, therefore, will consist in the perfect division of these structures; while failure will generally result from their incomplete section. This is so obvious that the mere allusion to it may be thought unnecessary; yet experience proves that of all the causes of failure this is the most frequent.

"Firstly. As regards division of the muscle, I have been witness to the following errors: the entire muscle has been left undivided, the thickened conjunctiva and subconjunctival tissue only having been cut through. This I saw happen to a child that had long suffered from strumous ophthalmia, and was affected with double convergent strabismus. The surgeon having, previous to the operation, boasted of his extensive experience and unfailing success in this branch of practice, proceeded to operate on both eyes; in one the muscle escaped division

altogether, in the other it was only partly cut through. The introductory remarks of the operator led me to expect a failure, and I was not disappointed. On seeing the child a few days afterwards, it squinted as badly as before. Again, the upper or lower margin of the muscle may escape division, or these parts may be cut through, and the centre left undivided. These shortcomings I have seen happen repeatedly, both in the subconjunctival and the ordinary methods of operating, whether done with the knife or the scissors. As respects division of the muscle with knives, on practising the operation on the dead subject, I found that M. Guérin's knife, which has a convex cutting edge, always divided the centre of the muscle, but was apt, without great care, to leave the margins undivided. Mr. Brooke's, on the contrary, which has a concave edge, insured the division of the latter, but sometimes left the former. For these reasons I adopted a straight cutting edge in my knife, or one very slightly concave. But, irrespective of instruments or modes of operating, a portion of muscle will sometimes be left undivided, and thus mar the success of an operation. Hence the warning cannot be too frequently repeated, never to terminate it without a most careful search with the blunt hook in every direction, a warning the more necessary now that chloroform is so frequently given.

But, secondly, every fibre of the muscle may be divided, and the eye still retain its abnormal position, from inflammatory adhesions. Of this I have met with two kinds; the one yielding, and capable of extension or rupture, by persevering lateral movements of the eyes; the other short, strong, and unyielding, incapable of extension or rupture, and requiring division by the scissors or knife. To the former are owing many of those cases of temporary failure or deferred success which most ophthalmic surgeons must have met with; and some have succeeded in remedying by fixing the eye by means of a ligature passed through the sclerotic end of the divided muscle. At the time of or immediately after the operation, these cases appear to be failures; but the eye subsequently rights itself, so that they ultimately turn out well; nevertheless, they have had considerable influence with many surgeons, in stamping the operation with a character of uncertainty.

A gentleman recently consulted me on account of a double convergent strabismus; he had previously seen other surgeons on the same subject, the majority of whom were adverse to operative proceedings. One frankly confessed he had never done the operation but twice, and never meant to do it again; for in one case he thought he had succeeded—in the other he considered he had failed. But, eventually, this state of things was reversed; the case that he took to be a failure turned out to be a success; and what was at first a success, ultimately became a failure. This evidence of the uncertainty of the operation was not to be resisted, and the surgeon has ever since been a staunch opponent of ocular tenotomy. With regard to the short strong adhesions before alluded to, they are fortunately rare. An eye thus fixed can never be liberated by any other means than their division. A most remarkable example of this kind is the following.

G. F—, æt. 14, came under my care on July 25th, 1857, for congenital paralysis of the third pair of nerves of the right eye. As he was anxious that something should be done to improve his appearance, I

determined, in the first place, to bring the cornea into a central position between the two canthi; and as I apprehended there might be some difficulty in getting it and maintaining it there, from the paralysed condition of the internal rectus, I passed an aneurism needle armed with a ligature underneath the external rectus, and then tied the ligature firmly around the sclerotic end of the muscle before dividing the latter; this having been done, the ligature was brought across the root of the nose, and the eye pulled in the same direction; the intention being to bring the cornea a little inwards, and to fix it there by means of the ligature. This, however, was found to be impossible, as it could not, by any justifiable force, be brought so far; and to maintain it, even equidistant between the two canthi, strong traction was required. I therefore divided the conjunctiva freely upwards and downwards, intending, if necessary, to snip the outer margins of the upper and lower recti; but it was soon apparent that these offered no obstacle to the movements of the eye, and that the real cause of its fixity depended on a number of short, strong, fibrous bands, intimately adhering to the sclerotic coat, and looking so like that structure, that I was once or twice in doubt whether I was not really dividing it; these were situated behind the vertical axis of the eye, and would just admit the point of the blunt hook beneath them. After they were all divided, and not till then, the eye became freely moveable, and could be pulled completely inwards, the cornea was then brought into the desired position, and the ligature, being no longer required, was removed.

Failure of an operation for strabismus may be owing not only to imperfect division of the structures engaged in its production, but to the implication of the opposite eye in the deformity. This was pointed out many years ago by Mr. Elliot, of Carlisle, who, in an excellent paper on the subject in the 'Edinburgh Medical and Surgical Journal,' set forth the grounds on which he was led to this conclusion, and illustrated its truth by many well-selected examples. He went, however, too far in affirming that strabismus can never be regarded as confined to one eye, and the facts on which he relied in support of this doctrine I have elsewhere shown must be differently interpreted. Nevertheless, the discovery of Mr. Elliot, that the persistence of a squint, after division of the adductor muscle of the affected eye, may be owing to the implication of the other eye in the deformity, led to a great improvement in practice; for surgeons had hitherto directed their attention solely to the worse eye, and if division of its adductor failed to rectify the squint, they proceeded to divide other muscles of the same eye, and the result was an exchange of one deformity for another equally unsightly. The inversion was, indeed, removed; but there was substituted for it either a bulging, staring, expressionless eye, looking nearly twice as large as the other, or the inward squint had been converted into one in an opposite direction. It will readily be imagined that results such as these contributed not a little to bring a very useful operation into discredit. The present practice, then, for which we are indebted to Mr. Elliot, is to divide the adductor muscle of the second eye as soon as we have ascertained, by a thorough exploration with the blunt hook, that no mechanical cause is in operation to prevent the rectification of the first eye. But failure of an operation may manifest itself, not only by a persistence

of the squint in the eye operated on, but by a transference of the deformity to the other and, apparently, unaffected eye. We operate, for example, on a patient supposed to be affected with single convergent strabismus of the right eye, we divide its adductor, and the eye becomes straight, it is bound up, and the patient sent home; on removing the bandage a day or two afterwards, the eye continues in good position, but the left eye is now observed to be inverted, and the operation gets the credit, or rather discredit, of having brought about this new deformity. Now, it is scarcely necessary to observe, that the inversion of the left eye is not really a new affection, for it was equally strabismic before the operation, though masked, so to speak, by the greater deformity in the right eye; and the disappointment which is always felt under these circumstances is owing to the surgeon not having recognised the doubleness of the squint prior to operating, and so failed to apprise his patient of the probability of a second operation being required on the other eye.

The failures I have hitherto alluded to arise from the operation being imperfectly done, and are therefore apparent at the time, or immediately after the patient has recovered from the chloroform, if that had been given him; but occasionally failure does not manifest itself till some days after the operation has been completed; and in this case it would seem to be due to some of the following causes: The reattachment of the divided muscle too near its original insertion. Thus Lucas affirms that he had several times been obliged to perform a second operation; and that, on these occasions, he was able distinctly to see both the original and the new insertions of the muscle. The same fact has also been observed by M. Lenoir, and is recorded by M. Berard. The muscle again may be united to the sclerotic, mediately, through the intervention of a band of condensed cellular tissue. Lastly, owing to the too free division of the conjunctiva, and the inflammation which sometimes follows, contraction may take place during cicatrization, to such an extent as again to draw the eye inwards. To whichever of these causes the reappearance of the squint may be owing, nothing can now be done except to redivide the shortened structures; but, as prevention is better than cure, I always direct the patient to commence practising lateral movements of his eyes on the third day after the operation. He will not do this violently, for it would cause pain, and, moreover, is not necessary; but I believe it to be perfectly effectual in preventing a relapse: at least, it has never happened in any case that I have operated on; and I am, therefore, disposed to attribute this success to the above practice.

ART. 107.—*A few remarks about Squint, especially External Squint.*

By Mr. HAYNES WALTON, Surgeon to St. Mary's Hospital, &c.

(*British Med. Journ.*, Aug. 21, 1858.)

“The class of cases in which the greatest relief can be afforded, is that in which one eye only is turned out, and the vision is but slightly affected; and the less this function is interfered with the more certainly will operative treatment be beneficial. But the great criterion for our

guidance in the selection of cases, that is, on which our prognosis should be founded, is the manner in which the eye can be righted, when the other is covered, or closed. When I find that it can be brought to the centre of the orbit, and maintained there; and, above all, if it can be so kept for a few seconds after the other has been opened, I strongly recommend an operation. I give equally strong assurances of success, when, without the sound eye being closed, the deformed one can be brought parallel with its fellow, and so kept for a short period.

"During this week I have had the satisfaction of examining a young woman, on whom I operated several months ago, through the recommendation of Mr. Ellison, of Windsor, whose case was of this latter favorable kind. A well-marked squint had existed eight or nine years, and the eye was presbyopic or long-sighted. I operated in expectation of the fullest benefit. An immediate improvement followed, but perfect parallelism was not restored. Now this partial effect is the usual occurrence in external squint; I looked for it, and with patient expectation awaited the completion. At the end of a week, when I dismissed her, the natural position had nearly been attained; and now, after the lapse of four months, I find as great perfection as can possibly be accomplished. I defy any one to tell, from mere appearances, that the eye had ever squinted, or that a surgical operation had been done. There is almost complete restoration of sight.

"Under less encouraging circumstances I often recommend surgical treatment, well knowing that even when the highest result cannot be accomplished, improvement will ensue. There are, however, cases, such for instance in which the eversion is so extreme, and the power of inversion so limited, and with which I have never failed to find a very defective retina, that I should decline to interfere. Three weeks ago I operated successfully, in private, in the presence of Dr. Sieveking and Mr. Farrant, on a young lady of eighteen, who had squinted two years. She could not see to read the largest type with the eye affected, nor could she recognise acquaintances; *but* she could bring the eye to the centre of the orbit, and maintain it there so long as she directed the sound one to any distant object.

"Concerning double external squint, the results of operations have been less satisfactory, because the circumstances under which I have operated, and which are those that are, I believe, usually met with in the double affection, are less promising. Yet when the eyes are involved in an equal degree, and either can be singly used, and made straight, the double operation may be confidently undertaken. Like the solitary deformity, there are less favorable states, which will yield less perfect ends.

"The last double operation I did at the Ophthalmic Hospital, but a fortnight ago, was no less pleasing to myself, and those of my colleagues that assisted me, than to the patient, a woman twenty-five years old, who said she had squinted since quite a girl. She used one eye at a time, and employed either apparently indiscriminately. She saw only distinctly enough to do coarse needlework, and blundered over small type, but could read large characters. In fourteen days after the operation she could read and work better, and the eyes were as nearly as possible in unison with their movements. Sometimes I detected a very

slight inequality, but so slight that it was not likely to be remarked by a casual observer.

"I operate in this manner: Having retracted the eyelids with the double spring wire retractor, I make, just over the insertion of the external rectus, a short vertical slit in the conjunctiva and the conjunctival tissue, which latter may be so thin as scarcely to be recognised, or so dense as to demand attention and carefulness in cutting through it. Then I introduce the blunt hook below the muscle, which I secure, and sever close to the sclerotica. I complete the process by bringing the edges of the conjunctival wound together by one or two sutures; if a single one does not effect the desired adjustment, I apply a second. This does not cause the slightest irritation, and possesses all the advantages that are to be got from the accurate closing of incised wounds, as compared with tearing them open. I do not practise the so-called subconjunctival division of the muscle, as suggested by Monsieur Guérin for internal squint, by which he hoped to overcome the dropping of the caruncle, and other defects arising from extensive dissections at the corner of the eye, so common at that time."

ART. 108.—*Case of Anophthalmos.*

By Mr. POLAND, Surgeon to Guy's Hospital.

(*Ophthalmic Hospital Report*, No. 4, 1858.)

Some doubts whether, in these cases, absence of the eyes truly exists, whether there is ever a real want of development or non-formation of the organs themselves, have been raised. Thus Walther* considers there is no true anopsia without acephala; he advances that it is to be regarded rather as a degeneration of the eye, such as is generally found, viz., a simple, red, undeveloped cellular mass, and would class these cases under the head of microphthalmos. But we have undisputed details of dissection, distinctly proving, not only absence of eyes, but also absence of optic nerves; thus at once determining that anophthalmos must be retained among the malformations.

The following case is one of absence of both eyes, as far as external evidence could be given. The child lived for some time, and was then lost sight of, so that in the event of death all chance of further investigating the contents of the orbit are frustrated.

CASE.—Some eight or ten years ago, I was called upon by Dr. Lever to examine a new-born infant respecting some malformation about the orbits and lids. The child was a male, born at full term; he was plump, well developed, and presented a perfectly natural conformation in every respect, with the exception of the lids. The orbits appeared somewhat smaller than usual; there were eyebrows over the superciliary ridges; there were no eyelashes, and the upper and lower lids were united along the whole extent of their margins, the union being marked by a reddish line; the contents of the orbits had a sunken appearance, seemed diminished in bulk, and the lids flattened.

A small aperture was made at the outer canthus, and a director was easily introduced along the inner side of the lids towards the inner canthus; the

* 'Lehre von den Augenkrankheiten,' Band ii, s. 242.

lids were then freely divided along the commissural line, and when separated, they were found to have but little adhesion to the contents of the orbit; there was no appearance of an eye or of a shrunken globe, for there was nothing to be seen or felt but an immovable slightly raised mass of thickened conjunctival membrane; an incision was made into this, but it only exposed reddish areolar tissue; there was no evidence of any muscles. The head appeared to be perfectly natural. The child was seen after three or four months, and then did not seem to differ from other children in general health and performance of natural functions. There were three other children of the family, but all perfectly natural. The mother persisted in attributing the defect to a severe fright she sustained when in the fourth month of her pregnancy; she was suddenly attacked by a dog who had lost one eye, which had a very ugly appearance; it made a great impression upon her at the time, and for some days after.

This case gives rise to many interesting questions, but unfortunately we are almost forbidden to draw any conclusions, as we have no positive proof that in this case there are no rudimentary organs; the eyes might have been fully formed and attained their normal condition up to the fourth month, and then have gradually ceased to grow, and become lost in the areolar tissue of the orbit. On the other hand, there may be no optic nerves, and no eyeballs developed at all. Thus then, on the subject of mental impressions, it is impossible to draw any deduction, for if there be no eyes and no optic nerve, an actual non-formation, the mental impression could have had nothing to do with it; if the contrary, then it is probable it may have had. There cannot be a doubt as to these effects of mental impressions; they have been ably argued, and many cases have been recorded.

ART. 109.—*On Microphthalmos.*

By Mr. WHITE COOPER, Surgeon-Oculist to St. Mary's Hospital.

(*Ophthalmic Hospital Reports*, No. 3, 1858.)

For some years Mr. Cooper has had under his care a family, three members of which present marked examples of this rare malformation.

CASES.—Mr. and Mrs. S—, the parents of the children whose cases I am about to relate, are healthy, well grown, and free from all ocular defect; nor has there been any known defect in the eyes of the ancestors on either side.

Mrs. S— states that when between four and five months pregnant with her first child, she met a man in the street whose eyes were so small and so peculiar that her attention was forcibly arrested, and, to use her own words, "this man's face was in my thoughts night and day until my confinement." Whether this really exercised any influence on the children afterwards born is an open question.

James S—, the eldest child, is now twelve. His height, 3 feet 11½; he has never had anything like a set of teeth; a few imperfect teeth have appeared, but soon dropped out; at present he has three ill-developed stumps. His frame is feeble, he is subject to headache, and is seldom free from discharge from his ears.

His eyes present the following peculiarities: the globes are extremely small, each palpebral fissure being but three fourths of an inch in length; the eyes are in constant irregular motion, and he experiences great difficulty in fixing them; the cornea in each is little more than two tenths of an inch in

diameter; the iris a mere irregular strip of a hazel colour, in parts altogether deficient, and without the slightest action; the lenses are clear, but there is an opaque spot on the anterior capsule in each eye.

When looking at an object, as the page of a book, he holds it close to his face very obliquely, turning his head from side to side with extraordinary contortions of the features; he always knits his brows, and looks from under them, being annoyed by light. His sight is decidedly dull, he makes out word by word, and sees distant objects very imperfectly.

The second child, Mary Anne S—, is now nearly nine. She has no teeth, those which have occasionally appeared having all dropped out. The condition of her eyes is very similar to that of her brother's. In her left eye the iris is entirely deficient along the inner border, and merely hazel-coloured shreds appear elsewhere; similar irregular shreds exist in the right eye; there is haziness of the external half of the right cornea; the left cornea is clear. The lenses appear clear, but an opaque patch is visible on the capsule in each eye. She frowns less than her brother, and holds objects at three inches' distance.

This little girl was examined two years ago by my friend Professor Tyndall, at the Royal Institution, and he reported as follows: "When the spectrum was displayed before her, she ran her fingers promptly over the colours, and named them correctly. The phenomena of irridiation presented themselves to her as they did to me; an incandescent platinum wire became thicker as she receded from it. As far as I could judge, the retina was perfectly healthy. I placed her within a foot of the coal-points of the electric lamp, and establishing the current, allowed the full splendour of the light to fall upon her eyes. She never even winked, but looked steadily into the light, and stated that she did not feel the slightest inconvenience. This, perhaps, was due to the partial opacity of the humours of the eye; the position of the iris in her case was marked by a few gray spots, and the pupil had no definite boundary."

Elizabeth Jane S—, the third child, is now seven. Her eyes are in all respects smaller than those of either of the others; the iris is deficient along the upper border in each eye, and that which exists appears merely as a grayish irregular strip. There are central opacities in the anterior capsules.

She has six ill-developed teeth in the upper jaw, and seven in the lower.

Her sight appears to be the best of the three; she frowns but little, and looks directly at a book, seeing well at about five inches.

Happily, the influence, whatever it might be, which led to this singular arrest of development in three children, exhausted itself with them; for there is a fourth child, a boy, one year and four months old, who is, in all respects, healthy, well-developed, and perfectly formed. The eyes are of full size, and he has already more perfect teeth than all the other children together.

These cases present in a marked degree the rarest form of microphthalmos, that in which irideremia, or congenital deficiency of the iris, is coexistent with the miniature dimensions of the globe; and we cannot but be struck with the experiments performed by Professor Tyndall, where the blaze of the electric light was borne by the girl without inconvenience, though there was no pupil to protect the retina from its influence.

Eight years have elapsed since I first saw the two eldest children, whose eyes were then really like dolls' eyes; and although a certain amount of growth has taken place in them in accordance with that of the frame, the microphthalmic character has been completely maintained.

In every case of double microphthalmos which has fallen under my notice, there has been imperfect dental development; and the same holds good in numerous cases of congenital cataract. The teeth in such cases are small,

jagged, discoloured, and soon decay. The cause which impedes the due development of the globe of the eye, or of the crystalline lens alone, influences the growth of the teeth. Nor does it rest there; for many of these children are stunted in stature, bow-legged, or knock-kneed, of wayward, irritable temper, and not unfrequently obtuse in intellect.

The paper also contains some other cases and remarks elucidatory of the causes of microphthalmos.

ART. 110.—*Six cases of Cataract in one family.* By Mr. STREATHFIELD.

(*Ophthalmic Hospital Reports*, No. 3, 1858.)

CASES.—On the 13th of February, 1858, I had an opportunity of seeing a family of the name of F—, living near Bury St. Edmund's, of which five children and their mother had double cataracts. It did not appear that any of the paternal or maternal relatives had been similarly affected. The grandmother, who was present, first observed the deficiency of vision, and the defect in the eyes of her child, the mother, when she was a year and a half old, and in the second generation, at various ages, *later in life, according to the seniority of the five children*; thus the eldest for six years, and the youngest for a year and a half, are said to have had good vision. There was not in either of the cases any history of short-sightedness before the defect was evinced, and after this time, *in a month to six weeks*, in either of the cases, the child became so blind as only to distinguish light objects. The six patients have all gray irides, and hair of a very light brown—in the younger children almost colourless. The mother has two sisters and a brother who have brown irides and good vision. All her children much resemble their mother in appearance, and all of the family, including the father (who is a labourer), are of a healthy, sanguine disposition, and have no other defects perceptible. In neither of the patients are there any corneal opacities, or unsteady movements of the eyes.

The Mother.

Ellen F—, æt. 33, has been incompletely operated on; the right pupil is occupied by remains of lens, and a wide margin of yellowish, earthy-looking deposit; the left is in the same state, but the outer half of the pupil is adherent to the lens capsule. With this eye, which is slightly turned in, she can only distinguish a light.

Her Children.

1. Laura, æt. 16; her sight failed her when she was six years old; has been operated on—remains of lens capsule.
2. *A boy, who died at the age of five years, without any known defect of the eyes or of vision.*
3. William, æt. 11; has been operated on—remains of lens capsule.
4. James, æt. 9.
5. *Eliza, æt. 7; read small print with facility, at an average focus; the eyes, examined externally and with the ophthalmoscope, showed no defect whatever.*
6. Henry, æt. 5.
7. Ellen, æt. 3.
8. *Mary, æt. 8 months, intelligent; the eyes, examined externally and with the ophthalmoscope, showed no defect whatever.*

Of these children, 5 and 8 were as much like the mother as the others were; and of the patients, 4, 6, and 7 (those which had not been operated on), the

cataracts were all of a gray opalescent appearance, most confirmed in the eldest, and least in the youngest. In this case, No. 7, the chief opacity was at the lower part of each lens, and left at the upper part a definite crescent, of the less opaque portion, largest in the right eye, which was said to have been latest affected. In No. 6 the cataracts were equal, not striated, and the general opacity was as great as that of the lower part of the lenses in the last-mentioned case, and greatest at the central part of each lens. In No. 4 the lenses were centrally most opaque, as in the two other cases. In this they were irregularly striated, and an additional peripheral opacity existed posteriorly in the right lens, and in the left posteriorly, and extending in front as a defined, superficial margin, to the deeper striated portion.

ART. 111.—*The influence of the cervical portions of the sympathetic nerve and spinal cord upon the eye and its appendages, illustrated by cases.*
By Dr. OGLE, Assistant-Physician to St. George's Hospital.

(*Proc. of Roy. Med. and Chir. Soc., June 22, 1858.*)

The main object of this paper is the application to clinical medicine of the various experiments which have from time to time been performed, as showing the influence possessed by the sympathetic in the neck and the upper part of the spinal cord upon the iris and upper eyelid. Experiments and dissection as regards the lower animals have shown that the curtain of the iris, containing as it does two sets of muscular fibres, a circular set by which the pupil is contracted, and a radiating set by which it is enlarged, is under the domination of two separate and distinct sources of innervation. The third cranial nerve is found to control the circular or contracting fibres, and the sympathetic, by virtue of communications with the lenticular ganglion, is found to control the dilator or radiating fibres. Hence, if the influence of the third pair be destroyed, the pupil becomes dilated, inasmuch as the dilator fibres, those presided over by the sympathetic, are unopposed; again, if the influence of the third cranial pair be left unimpaired, and that of the sympathetic be destroyed by section or extreme pressure, then the pupil becomes contracted. The author dwelt upon the history of the various experiments upon which the above statements are made, and also upon those from which it is concluded that in certain parts of the spinal cord resides the power or influence which acts upon the dilator fibres of the iris, passing to that structure through the sympathetic *viâ* the roots of certain cervical and dorsal nerves. From these latter it is apparent that the same paralysis of the dilator fibres of the iris which follows section of the sympathetic in the neck, follows also the severance of such fibres as connect the sympathetic with the spinal cord, as also the section or destruction of the spinal cord itself in certain parts. Accordingly, it might naturally be expected that any cause of extreme pressure acting upon the various portions of the nervous system before alluded to would, as in the various experiments before adduced, cause a contracted state of the pupil on the side corresponding to that on which the extreme pressure existed. And thus it was that Dr. Gairdner, of Edinburgh, first sought to explain those cases in which, along with an intra-thoracic aneurism, a contracted state of the pupil coincided. These cases were detailed, several of them not having been hitherto recorded, and to these

others were added from the author's own observation, as well as some from other sources. Cases were next given in which pressure from aneurism upon the sympathetic in the neck had produced contraction of the pupil. In the third place, instances were adduced in which extreme pressure from other causes than aneurism had produced a like effect upon the pupil, as in the case of enlarged glands, carcinomatous deposit, &c. In the fourth place, bearing in view the intimate connexion between the sympathetic main branches in the neck and the cervical part of the spinal cord, he drew attention to several cases in which a contracted pupil had been observed in injuries of the spinal cord itself.

But in addition to a contraction of the pupil as brought about by section of the sympathetic, spinal cord, &c., as before spoken of, experimenters have also found that irritation or galvanism of the same parts of the nervous system will bring about a dilatation of the pupil, and that this dilatation may be effected even when section or extreme pressure has already given origin to contraction of the pupil. Accordingly, in these physiological facts an explanation was sought of certain cases in which pressure from aneurism, diseased products, &c., appeared to produce, not a contraction, but a dilatation of the pupil in man; and he instanced, in the fifth place, several cases in which the pressure from various sources was inestimably so much in extreme as to be, in fact, a source of irritation or stimulus, acting in the same way as it was found in animals, that any stimulus, mechanical, chemical, or galvanic, would act upon the sympathetic. In no other way could he explain the dilated state of the pupil which existed.

But besides the above-described effect upon the pupil of the eyes, in enumerating the various experiments in which a sympathetic, &c., was divided, special attention was drawn to a dropping of the upper eyelid, or ptosis, which on several occasions was observed. This phenomenon was explained on the supposition that along with the sympathetic fibres to the iris, those to the third cranial pair are also paralysed, and hence the levator of the upper eyelid, which is supplied from the third pair, is deprived of power to a greater or less degree. One or two cases were also adduced in which ptosis of the upper eyelid was observed in connexion with pressure about the neck, from aneurism or other sources. He offered the same explanation of the convergent strabismus which, in the hands of certain experimenters, was, along with other results, found to depend upon a division of the sympathetic cord in the neck. He supposes it to have existed by reason of paralysis of such fibres (in several animals, five or six in number) as pass up to join the sixth cranial pair of nerves, by which the power of this muscle becomes weakened, and its action counterbalanced by the internal adductor muscle.

ART. 112.—*Vascular tumour of the Orbit successfully treated by injecting tannic acid.* By MR. HAYNES WALTON, Surgeon to St. Mary's Hospital.

CASE.—A lady, æt. about 20, was sent to me in the summer of 1856, by

Mr. Square, of Plymouth, for my opinion on a vascular tumour in her left orbit. The eyeball was prominent, and turned slightly upwards and outwards, restricted in motion, and incapable of being directed inwards. The lower eyelid bulged, and was slightly discoloured, especially towards the nose.

By turning down the tarsal margin, there was exposed a growth with a definite outline, having the appearance of a congeries of veins attached to the eyeball, and which was evidently only a part of a larger mass. Pressure emptied it, the act producing pain, and the redistension was very slow. The chief subjective symptoms were these: Pain on the slightest exertion, and in extremes of temperature, and during the catamenia. Inability to lie down, because of unpleasant sensations in the orbit and head. Vision nearly extinct.

The affection was congenital. Very soon after birth slight swelling of the lower eyelid was noticed. At five years of age, on account of increased tumefaction, Sir Benjamin Brodie was consulted, and according to the statement of the patient's mother, he did not consider it of any consequence. At the age of thirteen it attracted attention generally, and besides being larger, there was a bluish tint of the integument. Reading or writing for some hours always caused temporary enlargement, and heightened the colour. Messrs. Lawrence, Tyrrel, Travers, Dalrymple, and others, were then seen; and while they differed in opinion respecting the nature of the malady, they agreed in their advice, which was to leave it alone, as there were no very urgent symptoms and but slight disfigurement. After this, Mr. Square, at the urgent request of the patient, and with no idea of curing the disease, but only of lessening deformity, applied a ligature to a large tortuous vein which ascended perpendicularly from the margin of the orbit to the edge of the cornea. This operation was successful as far as it went, but excited very severe inflammatory action. I learn besides, from this gentleman, that our patient had a small nævus under the tongue and one on the thigh, both of which he removed; also, that many years ago, he successfully treated her brother for a varicose state of a single vein of the conjunctiva.

There was now more decided increase from year to year; the sight got very defective, there being often double vision, and the commencement of the other symptoms that I have described as existing when I was applied to.

I made a very careful examination, and came to the conclusion that the tumour consisted in chief of dilated and tortuous veins. I did not forget to scrutinise its relations, as on them, I was aware, should depend in a great measure the admissibility of doing anything, and not less the measures to be employed. I believed it was confined to the orbit. I had suspicions of prolongation into the lachrymal duct, but the integrity of the excretory lachrymal apparatus was not consistent with such a theory.

The position of the eyeball, often a means of determining the nature and situation of a tumour, induced me to think that it did not pass back into the orbit, at least not far back, but was limited to the inner and anterior part. Ultimately I recommended treatment. Further advice was sought. Among those applied to was Mr. Bowman, who alone coincided with me in recommending an operation. The lady was then placed under my care. It may be well to mention here that she was most desirous that some proceeding of a radical nature should be attempted, and had her wish not been gratified in England, it was the determination of her friends to proceed to the Continent.

I resolved to resort to the method of injection, and thereby to endeavour to produce coagulation of the contents of the tumour. The perchloride of iron seemed to be the most appropriate agent, and I had almost determined

to use it, but calling to mind some instances of sloughing that I had seen follow in its employment in *nævi* and other vascular growths, I hesitated, because, although in the instances alluded to there was no bad result and the severe local action was of no consequence on the trunk and on the limbs, in the orbit, on account of the close relation to the brain and its meninges, one of which communicates with this cavity, the greatest caution must be exercised lest the cerebral mass be affected. Not wishing, however, to relinquish this remedy, as it appeared otherwise suitable, and I had faith in its potency, I suspected that if used under certain precautions and in minute quantities, for instance, with the screw-piston syringe, it might be safe. I then endeavoured to ascertain its effect on blood out of the body, especially the quantity needed to produce coagulation, and when I found that a few drops rapidly and entirely coagulated an ounce, I rejected it as a matter of prudence, as I considered its action much too powerful.

I then experimented with a solution of lactate of iron, but I found that it was not capable of producing coagulation, which shows that the coagulating property of the muriated tincture is not due to the iron, as is generally supposed, but to the acid. While in search of an agent that could be depended on, Mr. Taylor, of Vere Street, suggested tannic acid to me, and it seemed to fulfil all that was needed. It did not possess caustic properties, and readily influenced blood, destroying the colour and reducing it to a sort of granular condition. These experiments were done in the presence of my friend, Mr. William Adams.

Having filled an Anel's syringe, prepared with the coarse pipe, with a saturated solution of tannic acid, I proceeded to operate, being assisted by Mr. Square, Mr. Adams, and Mr. R. Taylor, one of my colleagues at the Ophthalmic Hospital. I made a snip in the most prominent part of the tumour; blood escaped at once and very freely. As quickly as possible I threw in the injection, emptying the syringe. The cessation of hæmorrhage, and the appearance of the orifice, proved that coagulation had ensued, and the solidity of the tumour verified the extent of the action. This was on the 14th of October, 1856.

There was much general depression and prostration, with frequent retchings for twenty-four hours; but this was due to the chloroform, not however from any careless administration, as Dr. Snow was in attendance. The local effects were œdema and closure of the eyelids, serous chemosis of the conjunctiva and more prominence of the eyeball, slight swelling and redness of the integuments at the side of the nose and the upper part of the cheek. These increased for a few days, and in the highest stage were accompanied with much headache and fever, pain in orbit, and the appearance of spectra, prismatic colours and coruscations. As much of the pain was confined to the tumour, which now, five days after the operation, was very much enlarged, soft, and very dark—indeed, slate-coloured, as was also the surrounding conjunctiva—I incised it, but no fluid escaped, although some relief was obtained. But on the following day, a sepia-coloured discharge commenced. The eye was motionless. There was no perception of light, and the pupil was fixed, as it had always been. The progress two months later is told in the partially restored power of the muscles, especially the lateral and lower recti, reduction of the ocular protrusion, and of the size of the tumour. The coloured discharge continued.

Later an abscess formed, and pointed just under the margin of the lower lid, from which I evacuated healthy pus.

November 20th.—The movements of the eyelids were quite restored. All chemosis had disappeared. Less of the conjunctiva was discoloured, but that

yet affected as dark as ever. There was still discharge from the old incision in the tumour, but it varied in quantity, some days there was scarcely any, and the colour was less dark. Several times pain and local irritation preceded the escape of fibrinous clots, about the thickness of a crow-quill, and varying in length from a quarter to half an inch; and twice a small cretaceous mass, about the size of a mustard-seed, was discharged.

I watched my patient until the middle of January. There was no longer any secretion from the seat of disease, and all trace of the tumour was lost. The oozing from the abscess that I opened had also ceased, and but slight coloration of the conjunctiva remained. The eyeball had fallen back nearly to the natural plane, but its movements were not perfect; it could be directed downwards and outwards, naturally, but not turned fully inwards; nor could it be raised to more than half the proper extent. She could lie in any position, and all unpleasant sensations were lost. Vision did not return. I was not surprised at loss of power in some of the orbital muscles, and of paralysis of the retina. Both were partially manifested before treatment, being directly induced by the morbid growth, and were easily increased by the inflammation and tumefaction of the orbital tissues. Similar effects may ensue when the cellular tissue of the orbit is highly inflamed from any cause, as I know by four kindred examples.

I resolved not to publish my operation till time had tested its efficacy. A year has elapsed since its execution. There is not the least trace of the disease, nor is there evidence, except under close inspection, of the exercise of practical surgery. The tears are conveyed away naturally. The eyeball moves more freely than before the operation. My patient and her friends are highly pleased with the result. I can, therefore, with proper authority, speak of the result as a cure, and recommend a similar procedure under like conditions.

ART. 113.—*Case of Nævus in the left cheek cured by the injection of tannic acid.* By Dr. QUINLAN, Surgeon to St. Vincent's Hospital, Dublin.

(*Dublin Hospital Gazette*, Sept. 15, 1858.)

CASE.—Eliza B—, æt. 9 months, has a subcutaneous nævus on the left cheek, about midway between the angles of the mouth and jaw. Her mother states that she observed this immediately after birth; it was then almost imperceptible, but has gradually increased, until it is now somewhat larger than a shilling. It can be almost emptied by pressure. The skin covering it is healthy. Two veins of considerable size lead from it.

Saturday, August 21.—I introduced a cataract-needle into the upper portion of the nævus, and broke up its structure. I then inserted a very fine platinum canula, and, by means of a small platinum screw-action syringe, constructed for this purpose, injected a scruple of solution of tannic acid, of the strength of a drachm of acid to the ounce of distilled water. I repeated the same manœuvre in the two other most prominent parts, until the nævus became well distended. Coagulation quickly ensued, as was shown by the almost stony hardness which the nævus assumed.

Eight, p.m., same day.—Left side of the face considerably swelled; nævus somewhat inflamed. Ordered the part to be fomented with cloths wrung out of hot water. To take a little Hydrarg. c. cretâ.

August 23d.—The swelling of the face is quite gone down; the nævus

appears to be undergoing a kind of chronic inflammation; the epidermis over it is desquamating.

Friday, September 3d.—The site of the nævus is occupied by a tumour resembling the induration left after a boil. There is no trace of dilated vessels. The two veins before alluded to have almost disappeared.

September 7th.—The induration appears to be undergoing gradual absorption; the three openings made by the canula are healed up.

ART. 114.—*A case of Deaf-dumbness of more than twenty years' standing, greatly benefited.* By Mr. TOYNBEE, Aural Surgeon to St. Mary's Hospital.

(Proceedings of the Royal Med. and Chir. Society, June 22, 1858.)

CASE.—Miss L. L.—, æt. 23, consulted me in the early part of the year 1857. Her history was that, as a child, she had heard only some loud sounds, and was quite deaf to all conversation, her means of comprehending what was spoken being derived from watching the movements of the lips; and the sound she uttered appearing to be the result of her attempts at imitating the movements she saw. Upon examination, I found that the voice was heard when spoken into the ears. I recommended counter-irritation, and the use of a long elastic tube. At first, she could hear only from three to five minutes at a time. In a fortnight there was a decided improvement in the hearing power, and the ears began to experience a painful sensation when she was spoken to too loud. To use the words of her sister (who devoted herself to the poor patient in a way that only a sister or parent could), “during the third week the improvement was wonderful. This was not so perceptible with the tube, as in her hearing generally. Everything was so much louder to her, but not more distinct. The noise in the street now quite annoyed her, she called it dreadful, although when we arrived in town she did not notice it.” She left me at the end of a month's treatment, and I directed her to be spoken to daily, but only by words of one syllable, and she was to repeat these to herself. Then very simple sentences were spoken to her; these she heard, and then she was spoken to through the tube in the ordinary way of conversing. She heard and replied by speaking through the tube herself, so that she should be able to hear her own voice and modulate it. Her sister writes: “In October, 1857, she spent three weeks on a visit, and there she was seen by people who saw her first before she came to consult you; they said they should not have believed it possible for her to have improved so much in hearing and articulation; her mind, too, had come out so. During the three weeks she was at Harrogate, she had a complete holiday (*i. e.*, the tube was not used), and when she came back, the hearing had not retrograded generally, but she did not hear so well through the tube. Since her return from Harrogate she has gone on steadily with the use of the tube. Latterly (this was written on February 2d), for some weeks, I have talked through the tube daily an hour divided into three or four intervals. Two or three times, when much amused, she bore its use without fatigue half an hour at a time, and she said she could have borne it longer. Even when she has had a difficulty in comprehending what was said, I have never allowed her once to see my lips while talking through the tube. We spelt the words which she could not make out, and she never once failed to find them out by the aid of the ear alone. One afternoon she could not understand a single word of sentences she had distinctly heard in the morning. By degrees, however, she made out a word here and there, and in a few minutes

heard everything I said. She had been absorbed in writing a letter till a minute or so before the use of the tube. Several times now, I observed that she heard far *more* easily at the end of our talk than at the beginning. To her best ear I am obliged to talk in a high treble, not loud. The left ear requires a deeper, stronger voice. Great distinctness and slowness are necessary—a monotonous tone suits her best. The final consonant must be strongly uttered. She says she now hears them, but she never did so before—she notices the difference of touch in persons playing on the piano, and can often understand much that is said without seeing the mouth. A few days since she exclaimed—‘You are talking French.’ Lately, she has gained many new phrases, trying to apply those she learns in conversation, very often making wonderful mistakes. Not long ago she said, ‘that tree is a great assortment for the birds,’ meaning, ‘resort for.’ At another time she said, ‘I hope you will not think me liberty;’ meaning, ‘I hope you will not think I take a liberty.’ She began to read half an hour a day; it was hard work, although the book was written for a child. As hearing improved, articulation and intelligence improved; and, lately, I have often wondered at the change. We increased the reading to an hour, my sister constantly saying, ‘I feel as if something were coming to my mind,’ and expressing surprise that she could understand what she never could comprehend before. She now quite distinguishes between *my* mode of pronouncing and her own; and we never had the tube while reading to her, as I knew its use would have distracted her thoughts from her book. Occasionally, when the word was a very difficult one, she made it out, and then had the tube used to convince her of her correctness. Numbers of people have remarked my sister’s improvement. A lady saw her the first time in the beginning of August last, and she saw her no more till November; when she said to me in astonishment, ‘I could not understand a word your sister said in August, now I can understand everything she says.’ When I began your plans, I had to ask my sister as a favour to myself, to allow me to talk to her sometimes; she was annoyed, and then did not hear so well. Now, things are very different. She very often proposes the use of the tube, and says, she wishes you could know what we had done for her.”

ART. 115.—*On Hare-lip operations.* By M. GUERSANT.

(*Gaz. des Hopitaux*, No. 24, 1858; and *Med.-Chir. Rev.*, July, 1858.)

“The more operations for hare-lip I perform,” M. Guersant observed, at the Society of Practical Medicine, “the more I am convinced,—

1. That in operating for simple or double hare-lip, at any age, the result is almost always favorable.
2. That the operation for complicated hare-lip, at whatever age it may be undertaken, is only exceptionally successful.
3. That in the operation, whether for simple, double, or complicated hare-lip, we almost always fail when a complication supervenes in the shape of disease of the skin or other part.”

He added that he generally operated early, but waited until six weeks or two months after birth, being able by that time to judge whether the child is lively and well nourished, and also to have it vaccinated, and thus obviate a mischievous complication. In support of these opinions, it is to be observed, that some cases of simple hare-lip operations practised a few days after birth have failed because the infants, unable to nourish themselves, fell into a state of languor and died. Moreover, a considerable number of children

die within the first two months, even without having undergone any operation. If this is true of the operation for simple hare-lip, it is still more so with regard to complicated hare-lip; for in the latter, the mortality is greater in the earliest period of life than it is in simple hare-lip. Moreover, having to undergo a more laborious operation, these infants lose more blood; and as in some of them the operation has to be completed at several intervals, it is better to wait till they have attained the age of a year, when they are better able to support it.

Great care must be taken both before and after the operation as to the presence of other children who have or have just had any of the diseases of infancy, as whooping-cough, scarlatina, measles, &c.; affections likely to cause the failure of this, as well as of all other operations performed on infants. A child, thirty-two months old, with a double hare-lip, had been vaccinated by M. Guersant a few days before he intended performing the operation, and was carried to the door of one of the wards in which scarlatina prevailed, in order to furnish lymph for some of the children in the ward. The next day it was operated upon on one side only; but two days after it exhibited scarlatina, union failed, and the child died.

For some time past, M. Guersant has employed separate points of suture, having found that they less easily cut through the lips of the solution of continuity than the twisted suture. In the above case, although union was prevented by the scarlatina, the two points of suture being divided at the end of a week, it was found that the lips of the wound had not been cut through by the double threads which were employed.

ART. 116.—*On some severe forms of disease arising from retention of Decayed Teeth.* By Mr. CLENDON, Surgeon-Dentist to the Westminster Hospital.

(*British Med. Journal*, June, 1858.)

After describing very graphically the ordinary effects of diseased teeth, Mr. Clendon proceeds to speak of some of the more extraordinary effects—disease of antrum, fungoid tumours of the gums, facial neuralgia, facial paralysis, and spasm in certain forms, &c. These remarks are illustrated by cases, and of these we take one as well worthy of notice.

CASE 7.—*Abscess and paralysis of the face.*—R. W—, æt. 34, harness-maker, gave me the following history: "In January last (1857) I felt pain in the ear, which began to discharge; then an abscess formed in front of the ear; this was lanced at the Northern Dispensary, and it continued to discharge freely for a week or more. Having lost the hearing on that side, I went to the Ear Infirmary in Soho Square for two months, without any benefit. Discharge of the abscess through the ear continued until December. I then first began to feel severe pain when I attempted to move the jaw: this was followed by inability to close the eye, or to put out my tongue. In that state I came to this (the Westminster) hospital, in January last, and was admitted under the care of Dr. Reynolds."

From the constant pain and tenderness on pressure, Dr. Reynolds, suspecting some local cause of irritation, sent him to me, in order that his mouth

might be carefully examined. When I first saw him, there was great difficulty in opening his mouth; severe pain in the condyle when he attempted to move the lower jaw, and on pressure being made in that direction; he was unable to close the eyelids, to contract the orbicular muscle of the eye, to inflate the cheek, or to protrude the tongue beyond the front teeth, gasping when he endeavoured to do it. On attempting to whistle, the mouth and nostrils were drawn to the opposite side; the external meatus of the ear was so contracted that a fine probe only would pass; and through the orifice, on pressure being made on the cheek, pus oozed out. He had no toothache, nor decayed tooth that he knew of; but I found the teeth on the affected side encrusted with tartar, from want of use. This to me is always a suspicious circumstance. He attributed it to pain in the hinge of the jaw in eating, and to his inability to remove the food from the cheek, owing to the paralysis of the muscles. On passing a curved instrument under the gum, between two molar teeth, he felt some pain in one tooth; and, repeating the experiment with the same result, I determined to remove it. There was a cavity in it, quite out of sight, and almost out of reach, through which, I have no doubt, air had from the first freely passed, and given rise to all the mischief. On washing out the mouth, he at once expressed a sense of relief; found he could open his mouth easily; and to the surprise of all present, he could protrude his tongue to the full extent without any apparent effort, and also partially close the eye. The relief might be described as instantaneous. The inability to contract the muscles of the mouth, to inflate both cheeks, and raise the angle of the mouth, still continues; and, although it is asserted that in the severest lesions the nerves do not slough, I nevertheless expect some branches of the portio dura have been destroyed in the long-continued abscess, and that loss of power in some of the muscles, as well as of hearing, on that side, are permanent and irremediable.

The paper concludes with the following practical remarks:

"Now, in all these cases, whatever the symptoms, and whether the pain be severe, mitigated, or altogether absent, there is throughout but one indication—namely, an effort of nature to get rid of the offending body, which we, if we would endeavour wisely to assist and second instead of to counteract her, ought at once to seek out and remove.

"But here is the difficulty. The patient has a dread of the operation, and will run all risks, submit to any other course of treatment, or any amount of suffering, rather than undergo it. An old lady of seventy once told me that, from her earliest recollection down to the period when the last remaining tooth had worked its own way out, she had seldom been free from pain for a month together; and yet she could never summon courage to submit to the operation of extraction: in her own emphatic words, 'she felt she must die first.' But in severe cases, where there is great tenderness or pain, or where the operation would be more than usually painful and difficult to perform, chloroform will deprive the patient of all excuse, and prove a great blessing. It allows the practitioner to open and examine the mouth carefully, perhaps for the first time; and then, at his leisure, to seek for and remove all that he deems necessary. It is true that, owing to some few casualties, which from the first I anticipated, some persons have as much dread of chloroform as of the operation itself; but my experience in some 3000 cases, taken indiscriminately, and extending over a period of more than ten years, satisfies me that, although, like all powerful agents, it is

dangerous if misused, it can always be given with safety if administered with proper care. I can truly aver that, until the other day, when, in a case of protracted operation at the shoulder-joint, much blood being lost, syncope came on while the patient was fully under the influence of the chloroform, but which was detected and counteracted on the instant, I have not had one case which, during its administration or subsequently, ever gave me a moment's uneasiness.

"Often, however, there is another difficulty; and that is on the part of the practitioner. Perhaps he examines the mouth, and ascertains the cause of the pain to be the root of a tooth broken off and buried deeply in the gum, or he finds a number of roots clustered together, presenting no tangible surface for the grasp of an instrument, and difficult to reach even with an elevator. He sees they ought to be removed, but shrinks from the task; to use a familiar phrase, 'he does not like the look of them;' and therefore recommends some palliative mode of treatment, and patience. In point of fact, from not feeling quite equal to the emergency, or wanting confidence in the use of the instrument, he is reluctantly compelled to leave his patient to an indefinite period of suffering, and perhaps to some of the evil results I have already pointed out. This abnegation of his legitimate functions, arising purely from want of confidence in himself, has notoriously had the effect of driving this department of surgery into the hands of unqualified and often uneducated practitioners, styling themselves *surgeon-dentists*, and affecting to consider the mouth as their own legitimate domain, to which the surgeon has no claim. But, whatever the cause, the result is the same; for now the old story is repeated; the cheek swells, pus forms, and there is either a circumscribed or diffused abscess. This is poulticed, to promote absorption, or to assist resolution, as the case may be. Now, I cannot too forcibly insist that fomenting the cheek under such circumstances is the most erroneous step of all; for, should the abscess point outwardly, there will be a sinus from which pus may continue to flow, perhaps for a year or more, as long as there is any root or carious bone to exfoliate, leaving, when healed, a deep pit or cicatrix in the cheek, which disfigures the patient for life. This is not a pleasant alternative for a man, but a very serious drawback to a woman, in the preservation of whose good looks we all feel a natural interest. The tooth should be removed, when the pus will immediately flow through the aperture thus made; or, failing that, hot water, or hot bread and water fomentations, should be used *in* the mouth; and, as soon as possible, the abscess, however deeply seated, should be opened freely through the mucous membrane, and the pus allowed to escape *into the mouth*.

"In conclusion, I think no one will deny that it should be a part of a medical man's education to know how to deal with such cases; and I trust that all you who have kindly gone with me through these observations will have felt their interest and importance; and, seeing how much suffering and mischief may spring from so small a cause as a diseased tooth, you will sympathise in my earnest wish to obtain the diffusion of more enlightened views on this much neglected, and therefore little understood, department of surgery. Every day confirms my experience of its necessity, and strengthens my desire to see it accomplished. Many complaints, that come under the notice of the practitioner as diseases of

the body, are in reality diseases of the teeth. It is clearly impossible to treat the body as a whole, if we are ignorant of its parts; and I will venture to say there is no part much more widely and universally affecting the general system than that of the teeth.

ART. 117.—*On bleeding from the Ranine Veins in affections of the throat.*
By MM. MÉTIVIER and CHARRIER.

(*Bull. Gén. de Ther.*, Jan., 1857.)

After an interesting trial, these physicians speak very highly of this practice in all inflammatory affections of the throat. M. Métivier adopts the plan recommended by Paré, and makes a transverse puncture in one of the ranine veins. M. Aran, on the contrary, who has employed the same method of treatment in affections of the throat, exposes the vein first, by making a slit in the mucous membrane, and then opens the vein, one or both, according to the quantity of blood required, by a longitudinal incision.

ART. 118.—*On Excision of the Tongue for Cancer.* By Mr. SYME, Professor of Clinical Surgery in the University of Edinburgh.

(*Lancet*, Aug. 3, 1858.)

Mr. Syme has lately tried the effect of extirpating the whole tongue. As cancer frequently exists for a long while without extending beyond the tongue, and yet rapidly reappears after the affected part has been cut freely away, there seemed reason to think that if the whole texture showing this disposition to morbid action were removed, the patient might escape future trouble. Some reports had reached him of this having been done by cutting into the mouth under the chin, and then completing the operation by means of either a knife or the écraseur; but feeling assured that it was impossible to accomplish the object fully and satisfactorily through the imperfect access thus afforded to the root of the tongue, he resolved to divide the jaw at the symphysis, and then draw the two halves aside, so as to get room for accurate dissection and ligature of the vessels.

CASE 1.—G. S—, æt. 47, a shoemaker, was admitted into the hospital on the 11th of November, 1857, suffering under extensive cancer of the tongue, which was diseased throughout its whole extent, except towards the root. He stated that only five months had elapsed since the commencement of his complaint, or, at all events, since his attention was first called to it. There was no glandular swelling, or any other sign of disease.

On the 9th of December I made an incision through the lip, and extended it down towards the os hyoides, then sawed through the thick part of the symphysis, and completed its division by cutting pliers; next had the two halves held aside while I dissected backwards, so as to cut and tie the lingual arteries near the cornu of the os hyoides, and finally detached the tongue closely from the body of this bone. The edges of the wound were then brought together, and the patient walked stoutly to bed. Food was supplied by the injection of milk and beef-tea through a tube introduced into

the œsophagus, and everything went on so favorably for several days that I entertained no doubt as to the patient's recovery; but on the fourth day he began to complain of uneasiness in the chest, had a quick pulse, and became feeble. These unpleasant symptoms rapidly assumed a still more serious aspect, and terminated fatally on the seventh day.

On dissection, the wound, larynx, and trachea were found in a satisfactory state, but the lungs were thickly interspersed with small indurations indicative of recent inflammatory action.

In considering this case, I felt at some loss to determine whether the fatal issue had necessarily resulted from the operation, or was owing to circumstances that admitted of prevention. The patient was ascertained to have been addicted to habits of intemperance. The wound had been healed throughout its whole extent, so as to leave no room for the escape of discharges from the fauces; and from an early period after the operation experiments had been tried on the power of deglutition, with the effect of exciting a great deal of sputtering and cough. It seemed not improbable that, but for these unfavorable circumstances, the operation might have proved successful; and therefore, although certainly having no great desire to repeat it, I did not feel entitled to decline doing so, should a proper case present itself.

CASE 2.—A few weeks ago I received an application from the hospital at Northampton to admit a patient, who appeared a very suitable subject for the purpose. He was 58 years of age, a remarkably respectable, healthy-looking man, without the slightest sign of glandular disease. He had suffered for nearly six years from disease of the tongue, and in the early part of last spring had undergone an operation by ligature in the Northampton Hospital. In consequence of the disease returning, he afterwards went to London (Guy's Hospital), where no operation was performed, but some strong applications were employed. He then went home, with the tongue greatly swelled, and diseased throughout its whole extent, except towards the root, where the texture retained its ordinary characters, so far as could be ascertained by the finger.

On the 31st of July I performed the operation precisely as on the former occasion, but instead of closing the wound throughout its whole length left about an inch of it open at the lower part, and inserted a piece of lint to prevent adhesion. I also directed that no food should be given, except by injection through a tube. Again things went on favorably for a few days, and again I entertained sanguine expectations of success; but on the evening of the third day, symptoms of the same alarming character as in the first case showed themselves; so that next day I saw there was no hope, and felt prepared for the fatal result, which took place on the following day.

On dissection, the air-passages were again found free from signs of disease until they reached the lungs, where there were extensive indications of recent inflammation.

ART. 119.—*Three cases in which the Superior Maxillary Nerve was divided behind the Ganglion of Meckel, without injury to the nutrition of the face.* By Dr. CARNOCHAN.

(*American Journal of Medical Science*, Jan., 1853; and *Journ. de Physiologie*, April, 1853.)

The result of three brilliant operations upon the upper jaw is interesting to physiologists, in that it shows that the wound can cicatrize, and the nutrition of the parts to which the superior maxillary nerve is dis-

tributed go on healthily, after the trunk of this nerve has been divided behind the ganglion of Meckel. The result, indeed, is decisive against the opinion of those who attribute to this nerve, as well as to the other branches of the trigeminal, an essential office in carrying on the nutrition of the face.

ART. 120.—*On Extirpation of the Parotid.* By Dr. J. M. WARREN.

(*Boston Med. and Surg. Journal*, May 14, 1857; and *North American Medico-Chir. Rev.*, Jan., 1858.)

In a paper on "Tumours in the Parotid Region," Dr. Warren states his experience upon this subject in the following paragraphs:

"As to the practical question which is often raised, whether the gland can be removed without the ligature of the carotid, the result of my experience is this. The parotid gland has been removed by me in six instances, which are given below: three for scirrhus disease, one for erectile tissue, one for melanosis, and one for hypertrophy; in none of these was the great artery tied. The experiment of dissecting out the parotid gland in the dead subject has been frequently made by me, and with a little care this can be done in most instances, leaving the great vessels behind, although sometimes a small backward-projecting bit of the gland is left, and this has been observed to escape disease. But in scirrhus affections, where the gland undergoes a gradual induration, the vessels are frequently pushed backward, as they were in one or two of the cases here given. The above observation is confirmed by my friend and colleague at the hospital, Dr. Gay, who made similar dissections on the dead body to ascertain this point.

"In a case mentioned by Dr. J. C. Warren the carotid was cut at the end of the operation, and the jet of blood struck the wall. The vessel was secured, the carotid being compressed below, and the patient did well. In a second case for the removal of a scirrhus parotid, in which I assisted Dr. Warren, the carotid was divided and tied. Three days after, as the patient was straining at stool, the vessel gave way, and the blood struck the ceiling. He almost at once fainted, and the friends were fortunately sufficiently cool to place a sponge in the wound, and to check the flow partially. I was called, and at once cut down upon the carotid in the neck, tied it, and stopped the further effusion of blood. Bérard, in his monograph on this subject, mentions many instances of removal of this gland without ligature of the carotid."

The paper is completed by the history of the six cases above alluded to.

ART. 121.—*Novel method of extracting a foreign body from the Œsophagus.*
By Dr. DAVID RICE.

(*Boston Med. and Surg. Journal*, Dec. 3, 1857; and *North American Medico-Chir. Rev.*, Jan., 1858.)

CASE.—Mrs. Field, a lady, æt. 70, while eating chicken-soup, accidentally swallowed a piece of bone the size of an American quarter of a dollar cut

into a triangular form. The bone lodged in the œsophagus, about two inches below the top of the sternum. Thinking that it might fall into the stomach, she neglected to apply for surgical aid until the fifth day after the accident. In the meantime, she had swallowed neither food nor drink, both regurgitating into the mouth with every attempt.

I was called the fifth day, to try to remove the bone by surgical means. My first attempt was by a piece of whalebone, the extremity being perforated with numerous small holes, into which were fastened a dozen or more loops, about an inch long, made with small linen twist.

With this contrivance I failed, after many patient trials. I could readily reach the bone, but the loops did not fasten to any point of its angular form with sufficient permanency to enable me to extract it. I could even pass the piece of whalebone beyond the foreign body, and ascertained that it rested upon the posterior side of the œsophagus, standing perpendicularly, with two of its corners fastened into the gullet.

I finally took a piece of dry sponge about an inch long, and of such a shape, when dry, as to fill one half of the œsophagus. This I tied to the extremity of my whalebone-sound. Turning back the head of the patient, I passed it down the œsophagus, *in a dry state*, as rapidly as I dared to do, until I was certain it had passed beyond the bone. I then introduced a little fluid into the mouth, which quickly reached the dry sponge, enlarging it to twice its natural size, completely filling the gullet. I drew it out, and with it came the bone, much to my gratification and the patient's relief.

ART. 122.—*Practical remarks on Tracheotomy.* By M. GUERSANT.

(*Journal of Pract. Med. and Surgery*; and *Dublin Hospital Gazette*, May 15, 1858.)

M. Guersant's object in this paper is to describe the safest mode of performing this operation, and of promoting a successful issue.

The instruments employed by this surgeon we have frequently described; he has added to their number a tenaculum, an œsophagian forceps, and a caoutchouc probe, destined to serve as a conductor to the silver canula.

The object of the tenaculum is twofold; it assists materially in the ligature of whatever small arteries may have been divided, and furnishes, if required, a safe and prompt mode of fixing the trachea. With regard to this point, M. Guersant reminds us of the importance of the above indication, in the eyes of several eminent surgeons. M. Bretonneau having remarked that the instability of the trachea frequently causes it to move, so as to render its incision a matter of some difficulty, recommended to fix it in its position with the assistance of long needles, curved at their point, and furnished at their head with a ball of sealing-wax. For the same purpose, Mr. Liston used a simple hook, with which he fixed the trachea, after having laid it bare by a prolonged incision of the skin. M. Chassaignac went one step farther: finding it next to impossible, by the unaided pressure of the fingers, to steady the windpipe, on account of its elasticity and the numerous muscles by which it is surrounded, it occurred to him to secure the cricoid cartilage with a powerful tenaculum, the convexity of which being grooved, like the catheter used in lithotomy, might serve as a conductor, along which the knife could readily glide into the trachea. Tracheotomy, thus performed,

is an exceedingly brilliant operation; and we must say that M. Chassaignac has never had any reason to regret having imagined this *modus operandi*, which has also been resorted to with success by a distinguished young physician, Dr. Isambert. M. Guersant himself has also employed it with advantage, but he does not, however, consider the use of the tenaculum as absolutely necessary. In many cases he is of opinion that it may be dispensed with; and on the other hand, he thinks that by cutting at once into the trachea, through the skin and all the intervening textures, important blood-vessels may be divided, and amongst others, Neubauer's thyroid artery. M. Guersant acknowledges, nevertheless, the real utility of Chassaignac's tenaculum; and to render it still more efficient, he has modified its construction in the following manner. Having remarked that the common tenaculum twists the trachea to a certain extent, and exposes the surgeon to open it at the side, he caused the instrument to be bent at right angles with its handle, so as to act much in the way of Deschamp's needle. We may also add, with M. Chassaignac, that in the absence of a special instrument, a ligature passed through the trachea with a curved needle will sufficiently answer the surgeon's purpose.

Formerly, when the trachea was incised, and found to contain false membranes, it was cleaned with a little sponge borne upon a whalebone. Unfortunately, this instrument acted like a ramrod, forcing back the diphtheritic secretions, and merely displacing the impediment to the free introduction of air. This disadvantage has induced M. Guersant to abandon the sponge, and adopt the œsophagian forceps, with which he removes readily the false membranes, hooking them up in the same way in which he recently extracted a bean which had slipped into one of the principal bronchial divisions.

He also insists upon the necessity of adding to the double canula a caoutchouc conductor of a proper size. It is an excellent guide when the silver tube has to be inserted—an anxious moment, during which hesitation of any prolonged duration might bring on asphyxia; with the conductor no mistake of any moment can possibly occur. If the instrument enters readily into the windpipe, the expired air, passing through the apertures of the catheter, gives satisfactory evidence of its penetration into the proper duct; and, in the contrary case, the apparatus is withdrawn; and, supposing the too small dimensions of the wound to have produced the difficulty, the operator, by merely enlarging the incision, has every chance of complete success before him.

Both before and after the operation precautions should be taken upon which it is impossible to lay too much stress. The child should repose upon a solid bed, at breast height, the head thrown back, and the hinder part of the neck supported by a tight straw or linen bolster, so as to bring forward as much as possible the entire infra-hyoidean region. The co-operation of several persons is indispensable, in order to maintain the attitude of the patient, and aid the operator. In private practice, what help can be obtained must be utilised, but a cool and skilful assistant must be placed on the side opposite the operating surgeon.

According to M. Guersant, the stoutness of the child must give the measure of the incision to be made. It is, however, a dictate of common prudence not to extend the section too far towards the sternum;

to examine with the finger, during dissection, the inferior angle of the incision; and to keep a vigilant eye upon *arteria innominata*, which, in young children, rises with the act of inspiration. In the hospital this artery was once injured; in private practice one of the carotids was likewise wounded; and in several instances the *œsophagus*. When M. Guersant performs tracheotomy he proceeds as follows:—With one bold sweep he divides promptly the skin and subcutaneous cellular tissue, on the mesial line. After sponging the wound, the intermuscular space is sought for. The knife is then laid by, and, with a grooved conductor without a stop, the veins are pushed aside, the lips of the wound being kept well asunder with blunt hooks or lid-depressors. The muscles are separated in the same way, and the trachea is thus laid bare. If it be unsteady, and deeply seated, M. Guersant fixes it with the cricoidean tenaculum; if not, he simply supports the aerian tube with the left index, and, introducing the knife perpendicularly into its cavity, he divides, according to the child's age, from below upwards, two, three, or four of its rings.

After the incision, the child must cough up the blood which has flowed into the respiratory organs. Should its exit present any difficulty, it may be sucked up with a syringe. The dilator is then introduced, previously to placing the canula.

Since the substitution of the double instead of the single silver canula, and since the precaution has been adopted of placing before its orifice, as a kind of respirator, a muslin or woollen tissue, through which the air may be filtered, and by which the inhaled air is kept constantly in a state of warm moisture, the anxiety caused by the solidification of mucous deposits within the tube has been dispelled. It is, however, always desirable to modify as much as possible the circumambient atmosphere. Thus, in rooms heated by means of stoves, the air is generally too dry; it should be supplied with moisture, by placing on each side of the patient's bed, and renewing from time to time, large basins filled with warm decoction of marsh-mallow roots, the tepid vapour of which will assist expectoration.

M. Guersant does not cauterize the lips of the incision as a measure of precaution. Should they, after forty-eight hours, present any diphtheritic appearance, he touches them with lunar caustic; two days later he repeats the application, refreshing the wound, in the interval, with lemon-juice. He does not object to leaving the canula untouched during the two days subsequent to the operation, although it may be necessary to withdraw it before that period. Thus, if false membranes block up the trachea, the canula must at once be removed, and the morbid secretions extracted with the forceps.

Some patients proceed most satisfactorily after tracheotomy, and yet the food cannot be swallowed without, as it were, going down the wrong way. This singular system is occasionally noticed at a much later period, even when the canula has been removed. It seems caused by the fact that the epiglottis, having been for a long time fatigued by the presence of pseudo-membranous secretions, has lost the power of properly performing its functions. In order to recover its elasticity it requires repose, and in the interval the patient languishes and wastes away. This functional incapacity is sometimes relieved by the administration,

recommended by Professor Trousseau, of soups thickened with bread or vegetables, macaroni, or hashed meat, the deglutition of which appears easier than that of purely liquid nutriment. If that plan be unsuccessful, a caoutchouc tube must be introduced through the nostril into the œsophagus; and thus, three or four times daily, broth, milk, or chocolate may be admitted into the stomach. The chordæ vocales may, like the epiglottis, be more or less crusted over with false membranes; after the removal of the canula no air passes through the larynx, its cavity continuing to be obliterated by diphtheritic secretions. In such cases M. Guersant has derived much advantage from the following plan:—A small pledget of lint is tied with a double string, one of the ends of which is firmly fixed to an India-rubber catheter, which is carried upwards into the larynx. This little apparatus raises the epiglottis, and, through the pharynx, appears in the fauces; it can thus readily be passed several times up and down through the larynx, so as to remove the false membranes adhering to the parietes.

The canula should be definitively withdrawn only after the expulsion of these morbid productions, and when the air-passages have recovered their complete freedom. As a proof that this object has been attained, the child must be able to blow out a candle, and should feel no dyspnœa when the lips of the artificial opening in the trachea are brought into close approximation. It is then proper to take measures to close the wound. M. Guersant applies only a simple dressing, and never contemplates immediate healing. He merely keeps down granulation by the application of lunar caustic, and he frequently meets in society young ladies whom he had operated on twelve or fifteen years before, and who bear no visible marks of tracheotomy.

Such are the local measures required by this operation. The general treatment, hygienic and pharmaceutic, has not less importance, nor is it less deserving of the practitioner's attention. The little patients must be in good air, and kept warm. In this respect it is most necessary to avoid any portion of their drinks falling upon their clothes or their persons. The question of nutriment is also of the greatest consequence. After all operations, except in very rare cases, the patients require support; after tracheotomy necessitated by pseudo-membranous croup, this indication is an obligation of the most urgent character. M. Guersant at first prescribes beef tea, and milk-and-water; somewhat later, tapioca, arrow-root, groats, infusion of roasted acorns, &c. Further, it is evident that if any special treatment can be with advantage applied to diphtheritic disease, it is incumbent upon the physician to give his patient the benefit of such treatment; but we will postpone entering upon this interesting subject until our next number.

ART. 123.—*On penetrating Wounds of the Chest.*
By Dr. FRAZER, Physician in the London Hospital.

(*Proceedings of Royal Medical and Chir. Soc.*, June 22, 1858.)

The author considers that certain observations made, and since extended, by him, while attached to the general hospital in camp before Sebastopol, during the height of the siege in 1855, may be acceptable

to the Fellows of the Society. There are no records of the effects produced by, or the treatment adopted for, wounds of the lungs, in the ancient wars. The employment of gunpowder in warfare in 1346, in which year large guns were used at the battle of Cressy, and subsequently in 1382, when small arms were employed by the Venetians, effected a complete change in the mode of action between contending armies, and a characteristic variety in the wounds inflicted.

In collating the statistics of lung-wounds, the author has been surprised at the small number which had been brought into hospital in the Crimea, and also at the few cases given in the records of various previous wars, and, in pursuing the inquiry, at the rarity of cases reported in the various medical periodicals and journals; and the remarkable scarcity of morbid preparations of lung-wound in the professional museums of London, of Chatham, and elsewhere, warrant a conclusion that our sphere of observation on this very important subject has hitherto been very limited. The author explains this by alleging that a large, if not the largest, proportion of mortal wounds proceed from injury to the lungs. The men are struck down, and die rapidly and unnoticed. Out of the grand total of wounded throughout the Crimean war, viz., 12,094, there are returned under the head of "penetrating wounds of the chest" only 125, being little over 1 per cent.

The points requiring most attention in relation to chest wounds are—the mortality; the diagnosis; the prognosis; and the treatment.

First, as to the mortality. The author is strongly inclined to the opinion that lung-wounds are very fatal, and says that great doubt exists in his mind whether many of the cases reported as "wounds of the lung" were not wounds of the pleura only, and sometimes not even of that membrane; and he believes that many of the alleged wonderful recoveries from wound of the lung would have been disproved at death if a post-mortem examination had been always performed. On the other hand, when the pleuræ only are wounded, recovery frequently takes place. Various experiments by the author, and Dr. Richardson and others, fully establish these points. He records his warm thanks to Dr. Richardson for his able assistance in the conduct of the experiments narrated in the paper, in which he has freely incorporated many valuable opinions, suggestions, and deductions expressed by that gentleman.

The diagnosis and prognosis are next noticed; and the author enters fully into the general symptoms attending "wounds of the chest," drawing attention to the fact, that there is often little or no anxiety, dyspnoea, or other marked symptom attendant on such wounds. The contrast, in this respect, between "wounds of the abdomen" and "wounds of the chest," is remarkable; in the former there is present, invariably, great nervous agitation and sinking. In pursuing the diagnosis, the prime question is whether the substance of the lung be wounded or not; for in this the prognosis is seriously concerned. An answer is not easily given. Some physiologists hold that the lung collapses, whether its substance be wounded or not, whenever the pleural cavity is opened. The author himself, Dr. Richardson, and many writers, have observed the lung of the wounded to expand on expiration, and to contract upon inspiration: as this curious phenomenon was seen sometimes when the lung was wounded, and sometimes

when the lung was not wounded, the author concluded that no reliable practical deduction can be drawn from this very curious physiological fact.

The author next enlarges upon those symptoms which are usually accepted as, and by numerous writers positively affirmed to be, conclusive proof of the substance of the lung being wounded. Various cases are cited to show the necessity for caution in pronouncing a diagnosis on the apparent track of the ball.

He shows by several tables, the comparative frequency of *dyspnœa*, *hæmoptysis*, *emphysema*, *pneumonia*, and *the passage of air through the wound*, in the cases which he witnessed.

Out of 9 fatal cases in which the lungs were wounded, only 3 had *dyspnœa*. Out of 9 fatal cases, in which the lungs were not wounded, 3 had *dyspnœa*. Out of 12 cases of recovery, 2 had *dyspnœa*. The author considers that *dyspnœa* is a consequence of the inability, during inspiration, of a lung to follow up the expanding chest wall. It will be most intense, therefore, when the action of the thorax is free, and when, from an obstruction in the bronchial passages, the air cannot reach the vesicles, and the lung remains more or less expanded. But, when there is an opening in the chest-wall, and air can pass freely out and in by this abnormal channel, the lung having collapsed, there will be no effort to retain its normal position, and consequently there will be no *dyspnœa*. If this be the true explanation, we must, then, receive *dyspnœa* with great caution as a proof of lung-wound.

The author views *hæmoptysis* as a most deceptive sign of lung-wound, notwithstanding that almost every writer on the subject has regarded its presence as a conclusive sign of lung-wound. In 9 fatal cases, in which the lungs were wounded, only 1 had *hæmoptysis*. Out of 9 fatal cases, in which the lungs were not wounded, 4 had *hæmoptysis*. In 12 cases of recovery, 3 had *hæmoptysis*. Indeed, in those cases where the lung has been wounded, as verified by examination after death, the appearance of the portion of lung around the track of the wound would lead to the conclusion that the highly condensed portion of lung had acted protectively against hæmorrhage. When it does occur to such an extent as to threaten suffocation, it becomes pretty certain that the trachea and some large vessel have been opened. That hæmorrhage, taken alone, is no proof of lung-wound, is shown by its happening in cases of mere concussion or contusion.

Emphysema, contrary to the opinion of most writers, is a very rare consequence of lung-wound. Out of 9 fatal cases, in which the lungs were wounded, it occurred in 3 instances. Out of 9 fatal cases, in which the lungs were not wounded, it occurred in 1 instance. It was present in 1 case out of 12 recoveries.

Pneumonia may supervene, but not of necessity, as some writers assert, to lung-wound; but when it does approach, it is only after a lapse of some time, and cannot, therefore, be made available as an early means of diagnosis. Out of 9 fatal cases, in which the lung was wounded, it did not appear once. Out of 9 fatal cases, in which the lungs were not wounded, it appeared in 1 case, and that on the third day. Out of 12 cases of recovery, it appeared in 2 cases, on the fourth and twentieth days respectively. The post-mortem examinations revealed,

in several instances, appearances which many persons would have put down to the effects of pneumonia, but which the author considers to have been merely an intense congestion. In some of the experiments a degree of congestion followed, within a few minutes, the infliction of the injury, which might easily have been mistaken for pneumonia, in a hasty pathological examination. The author considers that when an opening exists in the chest-wall, the physical signs indicating the presence of pneumonia are so modified that no reliance can be placed upon them. He dissents from the opinion expressed by some authors, that traumatic and idiopathic pneumonia are homogeneous states, and gives his reasons for this difference of opinion.

The passage of air through the wound, often with a loud gurgling sound, and appearing to take the place entirely of the tracheal passage, has a most startling effect upon the bystanders, and is generally put down as a certain sign of lung-wound. Out of 9 fatal cases, in which the lung was wounded, it was present in 2; out of 9 fatal cases, in which the lung was not wounded, it was present in 4 cases; out of 12 cases of recovery, it was present in 1; it was present in 3 experiments in which the lungs were not wounded. The author is rather inclined to the opinion that when the lung is really wounded this "passage of air" will in most cases cease. Dr. Fraser concludes, that although there are none which can be regarded as special indications of lung-wound, yet, if there were three or more of the ordinary signs present, they may be taken as strong presumptive proofs of its existence; and if there be present, besides, more or less anxiety, coldness of surface, and orthopnoea, it may be considered nearly certain that the substance of the lung is wounded, and that the patient is in imminent danger.

The author enters fully into the treatment of "penetrating wounds of the chest," and inculcates the non-necessity for an over active manual interference to remove the lodged missile, by showing that a ball may remain innocuous for years in the thoracic cavity, and he gives, as one example, a case where the ball was fifty years in the body, and mentions an instance of a gallant officer, who, after having been subjected for some time to the well-meant but injudicious pokings of his surgeon, inquired what he was about, and on receiving the answer, "searching for the ball," his reply was gruff and graphic, "I wish you had told me that before, because you will find it in my waistcoat pocket."

The author next adverted to what has been, and is still asserted by many to be, the "sheet anchor" in the treatment of "penetrating wounds of the chest:" viz. venesection. He gives the opinions of others, and expresses his own doubts as to the prophylactic power of venesection in obviating the tendency to inflammatory action, or in arresting its progress, or in removing its effects when present. In reference to treatment, he recommends the removal of foreign substances, and all other causes of irritation, when practicable, from the wound. When the wound is small, and especially if there should be two openings, the closing of the anterior is to be attempted; and, if there be no sign of effusion, both may be closed; and, in all cases, absolute rest, cooling beverages, and moderate nourishment are called for, avoiding overstimulation. Bleeding, mercurialization, narcotism, and antimony, the

old elements of treatment, may, under the direction of sound skill and under special circumstances, become advisable; but their routine application is second only in mischief to the injury itself.

ART. 124.—*Case of Epithelial Cancer of the Œsophagus in which gastro-tomy was performed.* By Dr. HABERSHON, Assistant-Physician to Guy's Hospital.

(*Guy's Hospital Reports*, Third Series, vol. iv, 1853.)

“The consideration of the complete particulars of this case,” says Dr. Habershon, “lead to the conviction that, if the operation had been performed earlier, more permanent benefit might have accrued. It was done with comparatively trifling addition to the sufferings of the patient; it was effected with ease, without collapse or peritonitis; the thirst and sense of starvation were relieved in a degree which were scarcely anticipated. In cases where starvation equally advanced as in this case has been witnessed, death has taken place as quickly; and it is probable that had the operation not been performed, death might have taken place as speedily, if not more so. The patient would certainly have been deprived of the relief which for twenty-four hours he experienced. Under these circumstances it is urged, that if a favorable case be presented, the same operation be performed, but without waiting till life is almost extinct.”

CASE.—Walter H—, æt. 47, was admitted into Guy's Hospital October 8th, 1857, under my care. He had resided at Tunbridge Wells as a stableman, was of ordinary stature, light complexion, and moderately nourished. He stated, that for sixteen years he had had winter cough, but that he had never had dropsy. On admission there was considerable dyspnœa; the lips purplish; the pulse compressible, but regular; the chest was resonant on percussion, and the respiratory murmur indistinct; distant prolonged expiratory murmur was everywhere audible, with some sibilant râle; the voice also indistinct, and tactile vibration diminished. The heart-sounds were regular and normal; the expectoration frothy and moderately abundant. The abdomen was moderately full and rounded; the liver not felt to be enlarged; the legs were not anasarcaous, neither did the urine contain albumen. There was a small hard gland felt about the anterior margin and upper part of the sterno-mastoid muscle. He was ordered the julep of acetate of ammonia, with nitric ether and compound tincture of camphor, squill and blue pill night and morning; and meat diet.

After he had been in the hospital for a short time, he began to complain of severe pain in the throat during coughing; but, on carefully examining the part, nothing could be perceived. In a few weeks pain was also produced in swallowing, especially when solids were taken, the cough continuing unrelieved. On December 14th, he continued to suffer severely, and became more anæmic; the countenance was expressive of great distress, and the mind irritable; deglutition had become very difficult, so that he could only take fluid forms of food, and some stimulant. The cough also was very troublesome, producing very severe pain in the throat; it was violent, and small drops of blood were spirted out in the act of coughing; the expectoration was thin and watery. The chest continued resonant; respiration very feeble, on the left side indistinct, and the expiratory murmur prolonged; the larynx was free in its movements. Nothing could be seen in the throat,

except slight œdema and redness towards the right side. The gland at the angle of the jaw remained of the same size; the pulse was compressible; the tongue clean; the bowels confined. Various means were tried, as conium and carbonate soda, with hydrocyanic acid, steel, &c. The bowels were acted upon by colocynth and henbane, by magnesia mixture, or by injections. Counter-irritation was applied to the throat—hot water fomentation, or cataplasms, blisters, &c. The inhalation of steam afforded some relief, but still more the smoking of stramonium leaves. Tincture of aconite, applied externally, was also of some benefit. My friend and colleague, Mr. Cooper Forster, examined the throat for me, but could not detect any cause of obstruction. The patient continued during January and February without any improvement, the emaciation increased, and both respiration and deglutition became more difficult, especially the latter. Morphia occasionally given, and the stramonium inhaled, afforded partial relief. On again examining the throat, Mr. Forster felt below the epiglottis, towards the right side, a rounded tumour, which was evidently obstructing the commencement of the œsophagus, and he believed its surface was ulcerated. The respiration, although noisy and accompanied with a loud inspiratory sound, was not hurried, and sufficient air appeared to enter the larynx. The propriety of performing tracheotomy was discussed, but it was decided that no benefit would be likely to accrue from it. The examination of the growth in the throat was followed by temporary relief; and the patient was able, for three or four days, to swallow solid food. The stramonium and other means of relief, were continued; and nourishment was given in any form that he could take it. On March 1st the emaciation had very much increased. During inspiration a loud noise was produced in the throat; and this sound had for some weeks been increasing in intensity, so that he had been unable to sleep for several nights, on account of the “roaring,” as he termed it. His voice had become more feeble, but the cough had almost ceased; he could only swallow fluids, and those very slowly; his nourishment latterly had consisted of milk and rum, with eggs. Deglutition was much relieved for two or three days by two small blisters applied on either side the larynx; but it again became so difficult, that nutrient injections were resorted to. On February 22d, these injections, by allowing the throat to rest, enabled him in a few days to swallow with more comfort. The pulse had become very compressible and small; the bowels occasionally constipated.—March 2d. The respiration became more difficult, and Mr. Stocker had tracheotomy performed in the night. The incision was made as low as possible, but the trachea appeared flattened from behind, and the patient could not bear the tracheal tube inserted; when it was attempted he appeared to be quite incapable of breathing. The operation did not afford relief; and a deep-toned rhonchus could be heard in the lungs. There was no congestion of the face; the pulse very compressible; cough slight; he was able to get down his rum and milk, and some blanc-mange, &c. On the 9th he was breathing more comfortably; the opening in the throat was patent, and thin pus covered the red margins of the wound; there was also less noise on inspiration.—24th. The emaciation and prostration of strength increased; his bones appeared barely covered with thin skin, and the face expressive of starvation. He said “he was famished.” He endeavoured to relieve his distressing thirst by moistening the mouth; but for four days he had not been able to swallow a drop of fluid. The attempt to swallow at my request was preceded by much hesitation and preparation, and was followed by a paroxysm of severe coughing. The expectoration had changed in character, and become muco-purulent. On examining the chest sibilant râles were everywhere faintly audible. There was no dulness

on percussion, but preternatural resonance. The voice was very feeble, and scarcely audible; the pulse slow and very compressible; the tongue clean; the larynx was moveable; the gland at the angle of the jaw as before; the opening in the skin made during tracheotomy remained open, and the skin was undermined, there being evidently no power to repair the wound. The abdomen was exceedingly contracted, the pulsation of the aorta being visible, and the arteries most distinctly traceable. There was no evidence of enlargement of the liver, or of disease of the abdominal viscera. He complained of pain towards the right side, and tied a handkerchief firmly around him to relieve the sense of hunger. The skin was dry. He passed about a pint and a half of urine during the day. The sleep was tolerable; the mind clear and active. Nutrient injections of beef-tea, eggs and rum, thickened, if possible, with flour, had been given, at first four times, and then six times a day. Milk also was ordered, and \mathfrak{M} v of tincture of opium to be added to each injection. On the 25th he appeared to be sinking, and the rectum ejected the enemata almost at once. His hands were cold; but he complained of a sense of heat. It now became a question, whether life was to be allowed gradually to die out, or an attempt to be made by any other means for the introduction of food; the patient appeared to have chronic bronchitis, with epithelial cancer at the commencement of the œsophagus, possibly extending into the trachea, and death threatened from inanition. Three modes of relief suggested themselves—1st, the forcible introduction of an œsophageal tube; 2d, opening the œsophagus in the neck; and 3d, opening the stomach. In reference to the first, there was evidence of a growth at the commencement of the œsophagus; and the trachea appeared partially compressed, as shown in the operation of tracheotomy. The disease in the throat was probably of the form of epithelial cancer, and the passage of a bougie must have been constantly repeated. The great irritation and coughing produced by attempting to swallow, showed that the epiglottis was extensively ulcerated; or that there was a communication between the œsophagus and trachea, which would render the passage of a bougie very dangerous. In some cases of cancer of the œsophagus, a bougie has been passed into the pleura, and led to speedy death—and probably the passage of a bougie could not have been effected—this decided against the first proceeding. As to the second—opening the œsophagus—the most frequent seat of cancer in that tube being opposite to the root of the lung, about the third dorsal vertebra, and consequently beneath the position at which the canal could be opened, would have made the operation a very formidable, dangerous, and useless one. In reference to the third,—opening the stomach—this alone appeared to be the operation which could possibly relieve the patient. Wounds of the stomach, as that of Alexis St. Martin, the cases recorded by Mr. South, those by Dr. Murchison, &c., showed that life could be continued after fistulous communication had been thus made. The operations on the lower animals proved that it could be performed with some probability of success; such an operation would give a chance of prolonged life, where death was certain; and where the peritoneum was healthy, there was less danger than in abnormal conditions of that membrane. If life were prolonged only for a short time, and food introduced, there would be relief to the distressing thirst and the fearful sense of starvation: and lastly, it was evident that the patient was dying from inanition rather than from the disease, nutrient enemata being refused. On the other hand, however, I felt that the disease was probably of a cancerous character, and would sooner or later terminate life; that the operation was a hazardous and uncertain one; and that life might possibly be continued for a few days by a small portion

of the injection being retained. After carefully weighing these facts, I asked the assistance of my colleague, Mr. Cooper Forster, and if he considered the operation of opening the stomach through the anterior abdominal parietes, for the purpose of introducing food, a feasible and warrantable one, I decided that it should be attempted. The operation was accordingly performed by that gentleman, the steps of which he will describe; but the skill with which it was executed, the scientific coolness and care displayed, and the manner in which it was brought to a successful termination, all who witnessed the operation can confirm.—March 26th. The operation took place about half-past 2 p.m., and was borne without a movement on the part of the patient. The pulse, which before the operation was 62, and exceedingly compressible, rose to 116. Six drachms of milk with part of an egg were introduced through an elastic tube into the stomach. About twenty minutes past 3 about two ounces more milk and egg were introduced; he complained of feeling a sense of heat, but appeared comfortable. He was now removed to bed. At 4 p.m., the pulse was 120 and still very feeble; it was decided to introduce every half hour, if the patient were awake, two ounces of milk and egg, and every second time two drachms of rum with it. At 9 p.m. he was comfortable; there had been slight pain in the left side; the pulse was fuller, 124; the skin less parched; and he had slept occasionally for a short time. Messrs. Greenwood, Gayleard, Owen, and Tuck, kindly volunteered to remain with him in rotation, and their assiduous care and kind attention is sincerely acknowledged. During the night he had four hours of sleep; he passed urine, and there were three slight watery evacuations from the bowels.—27th. About 10 a.m. he coughed violently, and the contents of the stomach were forcibly ejected through the wound. His pulse continued 120. At 1 p.m. he was cheerful, his eyes more bright, his voice stronger, the skin less parched, his tongue moist, thirst and the sense of starvation relieved; he had pain in the left side; the pulse 120, and very compressible; his hands were cold, feet and legs warm, the coldness of the hands was very marked for several days before the operation. The operation had evidently mitigated his suffering. At 1:30 p.m. half an ounce of rum, with sugar, and an ounce and a half of water, and fifteen minims of lemon juice were given. The stomach received it well, contracting upon the tube. He said that it produced a comfortable sense of warmth throughout the abdomen. At 3:30 the pulse was firmer and fuller than at 1 o'clock, and the hands warmer. Since the operation, during the twenty-four hours, he had six eggs, beaten up in twelve ounces of milk, given in small, divided doses, with four ounces of rum. Milk and egg, or beef tea thickened with flour, were ordered every half hour, and occasionally half an ounce of rum, as just mentioned. At 8:30 p.m. faintness came on, the face became cold and perspiring; pulse 136, and scarcely to be felt. The stomach appeared to have lost its power of contracting on the food introduced.

Stimulants were ordered to be given repeatedly and freely, with nourishment as before; and two or three times, as a stimulant, \mathfrak{mxx} of tincture of sesquichloride of iron. During the night he was evidently sinking, the pulse sometimes became scarcely perceptible, but rallied after stimulants were introduced. On the 28th he slept for a short time about 10 a.m., and expressed himself as comfortable; but gradually became unconscious, and died at 10:45. rather more than forty-four hours after the operation.

Inspection was made twenty-eight hours after death. The body was extremely emaciated. The head was not examined. At the lower part of the neck, immediately above the sternum, was the wound made in tracheotomy, gaping and undermined, and on the trachea a few drops of pus. At the left

hypochondrium was the opening made by the operation of gastrotomy, also enlarged by the plug which had been introduced a few hours before death. The mouth and soft palate were healthy, also the epiglottis. At the posterior surface of the cricoid cartilage there was a growth connected with the mucous membrane, about a quarter of an inch in elevation, and extending from side to side, soft and slightly injected; passing downwards, there was irregular ulceration, and towards the trachea destruction of all the coats of the œsophagus; on either side and below, the ulcer was bounded by a sharp undermined edge. The cellular tissue of the trachea and its muscular fibres were destroyed for about half an inch; the mucous membrane was bare, and perforated by a small opening about one sixteenth of an inch in diameter, so that fluid could pass from the œsophagus into the trachea; below the ulcerated surface in the œsophagus the canal was much contracted by infiltration into the surface of the mucous membrane; the passage was so much diminished at this part that a probe could only be passed after death, and it was probably quite impervious to fluids during life. The constriction *was situated at the level of the first bone of the sternum*. The rest of the œsophagus was healthy. One or two glands in the neck were infiltrated and diseased, but none of the mediastinal or other glands. The rima glottidis was free; the vocal cords and aryteno-epiglottidean folds quite healthy; so also the trachea. The bronchi contained thick tenacious mucus. The pleura on the left side was healthy; on the right, there were general, but not firm adhesions. The lungs were both much distended with air, pale, emphysematous, and covered the heart. At the right apex the lung-tissue was puckered; there were numerous lobules of iron-gray consolidation, with intervening crepitant lung, but no disorganization. The lower lobe of the right lung afforded a beautiful specimen of emphysema, but there were numerous gray tubercles studded in small clusters; they were non-cancerous. The lower lobe of the left lung was much congested, and one or two lobules were softened and breaking down, from acute changes, probably a very short time before death. There was no enlargement of the bronchial glands. In front of the surface of the heart was a small collection of pus, only a few drops, apparently from the inflammation of small gland. The pericardium and heart were healthy, the heart contracted and firm. On opening the abdomen, the intestines were found contracted; *the peritoneum was healthy; no inflammation, effusion of lymph or serum, or diminution of its normal smoothness could be detected*. The stomach was partially distended; it was situated lower than usual, and its anterior surface was looped up to the opening in the anterior abdominal parietes made by Mr. Forster at the linea semilunaris. The mucous membrane of the stomach was pale, slightly injected at the opening. On gently drawing aside the stomach at the opening, the opposed serous surfaces were found slightly adhering. The small intestine was healthy throughout, but atrophied; the food introduced had only passed four feet down the intestine; below that point the intestine was exceedingly small. The lower part of the ileum was healthy. In the colon there were several patches of congested mucous membrane. The gall-bladder was distended, the liver healthy, so also the kidneys; the spleen was very small. There was no evidence of any cancerous disease affecting any part except the œsophagus and one or two adjoining glands.

ART. 125.—*On Rupture of the Stomach and Bowels resulting from Contusions of the Abdomen.* By Mr. POLAND, Surgeon to Guy's Hospital.

(*Guy's Hospital Reports*, 3d series, vol. iv, 1858.)

In this valuable monograph, Mr. Poland relates 66 cases, which exhibit very clearly the variety of symptoms which may occur in this class of injuries, and the almost uniformly fatal result. In these cases, indeed, 64 were fatal, and the remaining two were cases in which perhaps the rupture may be considered as questionable. On giving a general idea as to the duration of life in these 64 cases, we take the accompanying table, with the remarks appended to it:

Duration of life.	Stomach.	Duo- denum.	Je- junum.	Ileum.	Small in- testine, not spec- ified.	Do., com- plicated with hernia.	Blow on hernial sac.	Large in- testine.
Few hours	2	1	1(?)
3 "	1
4 "	1	...
5 "	1	1	1	1
7 "	1
12 "	1	...	1	1	...
13 "	...	1	1
16 "	1
18 "	...	1	1	1
21 "	1
22½ "	1
24 "	4	1	...
28 "	1
30 "	1	2
31 "	1
34 "	1	...
36 "	1	1	1	...
42 "	1	1
48 "	2	2	4	1
3 days	...	1	2	...
4 "	1	...
5 "	1	1
6 "	1
8 "	1
16 "	1
2 months	1
Not stated	1	3	1	1	1
	3	4	14	16	6	3	13	5

‘ In drawing any conclusion from the above 64 cases, we must omit

7 cases, in which the time of death is not stated; and we must also exclude the case in which death did not take place until the second month. inasmuch as the case was one of recovery:—thus, then, we have 56 cases for comment: of these,

“10 cases were fatal in the first five hours or stage of collapse, being 17·85 per cent.; of these 2 were of the stomach, 1 of the duodenum, 3 of the jejunum, 2 of the ileum, 1 of the bowel in a hernial sac, and 1 of the large intestine.

“18 were fatal from five to twenty-four hours, during stages of primary and secondary effects, being 32·14 per cent.; of these 2 of the duodenum, 4 of the jejunum, 6 of the ileum, 1 of the small intestine (not specified), 2 of the intestine complicated with hernia, 2 of the bowel in hernial sac, and 1 of the large intestine.

“Thus, within the first twenty-four hours, 28 cases, being 50 per cent., or half of the whole number, proved fatal in this period.

“19 cases were fatal from twenty-four to forty-eight hours, during the acute and subsequent stages of peritonitis, being 33·92 per cent.; these include 6 of the jejunum, 5 of the ileum, 6 of the bowel in the hernial sac, and 2 of the large intestine.

“9 fatal between the third day and the sixteenth day, during the reparative attempt, being 16·07 per cent.; of these 1 was of the stomach, 1 of the duodenum, 1 of the jejunum, 2 of the ileum, 1 of the small intestine (not specified), and 3 of the bowel in hernial sac.

“In concluding this imperfect attempt at drawing the attention of the profession to this class of injuries, we have been mainly influenced by the fact that much injudicious and careless (we might almost say reckless) treatment has been resorted to. In the numerous instances here collected, we have sad specimens of the action of purgatives in hurrying the unfortunate patient to a more speedy and inevitable death.

“In more than one or two cases has the castor oil administered by the mouth, been found floating about in the peritoneal cavity.

“Hence, then, how cautious should the surgeon be, when called to any case of injury to the abdomen, of however slight a nature it may be? The treatment in all such cases should be that which has been laid down in the early part of this communication, viz., perfect rest of body and perfect rest of bowels; no solid or fluid to pass into the stomach for at least forty-eight hours, and then only in small quantities, so as not to undo what slight reparative attempts have been made by nature: in some instances nature had almost completed her repair of the breach, when by injudicious treatment the temporary barrier has been forced asunder, and speedy death followed. And while again referring to this subject, we will conclude by adducing the following case:

“Only a few weeks ago a boy was run over by the wheel of a carriage, and the mark of the track of the wheel was distinctly seen extending obliquely across the abdomen. The child was brought to Guy's in a collapsed state, became extremely restless and tossed himself about, evincing great pain and agony; he presented every symptom of ruptured bowel: he remained collapsed for several hours, and had nausea, &c. The boy was starved for forty-eight hours, and had opium in powder,

and allowed to suck small quantities of ice at prolonged intervals. He recovered. The case, perhaps, was one only of shock without internal lesion; still it serves as a good typical case of not interfering in such injuries."

ART. 126.—*Intense thirst a symptom of displacement of the Stomach in Diaphragmatic Hernia.* By Dr. WILKS, Assistant-Physician to Guy's Hospital.

(*Lancet*, Oct. 23, 1858.)

The presence of this symptom in the three following cases, in Dr. Wilks' opinion, warrants the conclusion that it is of importance in diagnosis.

CASE 1.—Henry S—, æt. 37, was admitted into Guy's Hospital on July 7th, 1855. While working on the Crystal Palace Railway, about two tons of earth fell upon him, producing great injuries to his chest and pelvis. On admission, his breathing was so short that it was thought he would expire every moment; the left side of the chest was enlarged, tympanitic, and emphysematous, the ribs being also broken. He survived the accident twenty hours, and his most remarkable symptom was *intense thirst*. During the time he lived, he drank immense quantities of water, but he did not vomit. The post-mortem examination revealed several injuries, which I need not here detail, but the most remarkable appearance was the immensely distended stomach, filling the whole left side of the chest. This, together with a portion of colon, had passed through a large rent in the diaphragm.

CASE 2.—In the '*Lancet*' of Jan. 19th, 1856 (I quote from a note, not having the journal by me), Dr. Fraser, amongst a series of cases reported from the seat of war in the Crimea, relates the case of a man who received a wound of the diaphragm, whereby the stomach was forced into the chest. He lived six days, but it was probably only on the last that the displacement occurred. The *thirst* on this day became excessive, and his plaintive moanings at not having as much cold water as he desired were painful to hear. He drank a large quantity a few minutes before death.

CASE 3.—This is the case you give in the '*Lancet*' of the 16th inst., in the report of which I find, amongst other urgent symptoms from which the man suffered, on one day mentioned—a great deal of *thirst*: on another day—drank a large quantity of water; on another day—drinks an *immense quantity of water*; and so on.

"That this thirst is more than a coincidence is shown from the fact, that, having the first two cases in view, I read through the last one carefully, in expectation of meeting with this symptom, and was not surprised to find it present. To be able to predicate a symptom in any given case is sufficient to prove its intimate connexion with it. It is rather a question for the physiologist to solve, why an inordinate stretching or distension of the stomach should give rise to this intense thirst."

ART. 127.—*A new operation for the radical cure of direct Inguinal Hernia.*

By Mr. WOOD, House-Surgeon to King's College Hospital.

(Lancet, May 29, 1858.)

The main features of this operation consist: 1st, in its being conducted subcutaneously, insuring a more ready and less painful healing; 2d, in the introduction into the canal of the two layers of superficial and intercolumnar fasciæ, which are unusually abundant and strong under the condition of hernia, and which are made to cohere into a solid plug by adhesion of their apposed surfaces, made raw by the subcutaneous separation from the skin, and are also supported and kept in position in the canal by the new adhesions contracted below by the skin from which they were separated; 3d, in the drawing together and close union of the sides of the enlarged external ring by the lateral traction of the ligature upon them, caused by its passing through the same opening in the skin and in the compress upon it, and tying them down by consequent permanent adhesion to the invaginated plug of fasciæ behind them; and 4th, in the firm compression made upon the part during the formation of the adhesions, and their consequent greater extent and firmness, by the traction of the ligature upon the boxwood compress.

Mr. Wood is of opinion that the distinct evidence as to the position of the hernial sac of peritoneum and of the cord through the opening in the skin, and the great certainty that the feel of the tube gives that its position at the extremity is close behind the aponeurosis of the external oblique, and has no intervening structure to be avoided between it and the surface, remove almost entirely the danger of puncturing the sac in this operation; while he supposes that the succeeding pressure may operate after a while in producing adhesions of the opposed surfaces of the doubled-up sac. The operation leaves no dimple or deformity whatever; the cicatrices are very small and slight, and after a time will become nearly imperceptible.

CASE.—John C—, æt. 25, a printer, applied at the hospital with a direct inguinal hernia on the right side, to which he had been subject eighteen months, with frequent obstruction and constipation of the bowels, and after meals had suffered from great pain and distress in the part. He has tried seven trusses, none of which had power to retain the bowel. The last he tried produced such irritation as to give rise to a series of abscesses in the groin, which was the direct cause of his application. The abscesses being healed, and the patient disposed to submit to an operation for permanent cure, the following was performed:

On examination, he was found to have a hernia projecting into the scrotum, producing a tumour, which he said sometimes reached the size of his two fists. On reducing it, the external ring and the internal opening were found to be enlarged, so as to admit the ends of three fingers, the margin being lax and loose. On the slightest cough or exertion the bowel immediately dropped down into the scrotum.

Description of the instruments.—The instruments used in this operation consist of: 1st, a tube two inches and a half long, mounted on a strong handle, about three inches and a half in length, curved in a circle of an inch and a half radius, and flattened into an oval at one end, and forming a linear

aperture a quarter of an inch wide at the point; 2d, a strong needle, having a corresponding curve, with a perforated point, projecting a full inch beyond the end of the tube when passed through it, and mounted on a strong handle; 3d, a box-wood pad or compress, two inches by one and a quarter, perforated by a hole at half an inch from one end, and crossed longitudinally by a bar of iron-wire screwed on to the upper surface; 4th, a subcutaneous section-knife, with a sharp point, a narrow blade, and an inch of cutting edge.

Operation.—The patient being laid on his back, with the legs a little drawn up, and the hernia returned, an incision, about three-eighths of an inch long, was made through the skin only, over the cord, about an inch and a half below the external ring, with the subcutaneous knife, which was then carried close under the skin, so as to separate a circle of the superficial fascia around the opening of two inches in diameter. The detached fascia was pushed up into the inguinal canal by means of the curved tube, the end being placed through the opening in the skin. The extremity of the tube was then carried behind and close to Poupart's ligament, or the external pillar, to the extent of an inch and a half from the pubic spine. It was then felt, by depressing the handle, to raise the external pillar upon the extremity. The needle, carrying the thickest silk ligature, was then protruded through the tube, and pushed through the external pillar and the skin, the latter being previously drawn considerably downwards and outwards. The needle was then withdrawn, leaving one end of the ligature on the surface. The end of the tube was next shifted upwards and inwards, and made to protrude behind the internal pillar, as far as possible from its margin. Through this the needle was then passed, and the skin moved upwards and inwards till the point appeared at the opening previously made. The ligature was then freed, and the needle withdrawn, the tube being still held firmly in its position. The ends of the ligature were next passed through the hole in the box-wood compress, one on each side of the wire bar, over which they were then drawn close and tied firmly, so as to retain the pad close down to the end of the tube in the canal, which was then withdrawn. It will thus be seen that the first passing of the needle pierces not only through the external pillar of the external ring, but through the origin of the internal oblique and cremaster muscles, from Poupart's ligament behind it; and, at the second passing, it goes through, not only the internal pillar, but also the conjoined tendon of the internal oblique and transversalis muscles, which is placed behind it, so that the sides of the inguinal canal are drawn together from end to end by the ligature. The wound in the skin of the scrotum was drawn firmly together by plaster, a fold of linen placed upon it, and the whole secured by a spica bandage: the patient being ordered to remain in bed, and to use no exertion whatever.

The bandage was removed on the third day, when the subcutaneous puncture was found completely healed by the first intention. The compressed ligatures were retained till the fifth day, some œdema and suppuration having by this time appeared around them, with considerable soreness and pain in the groin. The opening for the ligature looked red, healthy, and suppurating. It was dressed with wet lint, oil-silk, a large compress, and spica bandage. In a fortnight it was completely healed. No irritation nor swelling of the testicle occurred in the course of treatment, nor any symptom of peritonitis.

At present, three weeks after the operation, the external ring is felt by the finger, pushed up beside the cord, to be completely blocked up by a broad band of fascia passing across it and up along the canal, with the cord passing by the lower part. The sides of the canal are felt adherent and consolidated. There is no *ballotement* whatever felt on coughing; the groin of the side

operated upon being more firm and resistant, in fact, than the opposite, which has always been a little weak. The patient was ordered to wear a truss for some months, to consolidate the part and protect the newly-formed tissues. The skin is firmly adherent to the subjacent structures at the point of the punctures and subcutaneous separation. He has since undergone a severe test as to the efficacy of the cure, in an attack of bronchitis, from which he has completely recovered, without in the least affecting the site of the hernia.

ART. 128.—*Case of ligature of the common Iliac Artery.*

By Dr. W. H. VAN BUREN, Surgeon to the New York Hospital.

(*New York Journal of Medicine*, Jan., 1857.)

CASE.—A professional gentleman, æt 46, unmarried, of irregular, intemperate habits, and obese in person, first consulted Dr. Van Buren, in May, 1853. He had noticed a tumour, then about the size of a hickory-nut, in the fold of the groin, over the course of the main artery, immediately below Poupart's ligament. The disease appeared to be spontaneous in origin.

"The tumour was now about the size of an ordinary child's head at birth, somewhat flattened upon its surface, occupying the fold of the groin, and extending upward along the course of the external iliac artery, beneath Poupart's ligament, the position of which was marked by a depressed line dividing the convex surface of the tumour into two equal halves. Its bulk was diminished very considerably by pressing the external iliac artery against the brim of the pelvis, and its pulsations were entirely checked by the pressure. The patient's abdomen was very fat, and continued pressure caused so much pain that I could not satisfy myself that the contents of the tumour were entirely fluid; but, from its rapid diminution, and also from its well-marked expansive pulsations, I concluded that it contained but a small proportion of coagulum.

"He was urged to submit to an operation, but hesitated and procrastinated until the latter part of November, six months longer, during which time the tumour had considerably more than doubled in dimensions. His aneurism had increased to a fearful size; it was still pulsating furiously, and its contents, as far as I could judge, were yet mainly fluid. Upon the most prominent part of its surface (above Poupart's ligament, where the great bulk of the tumour now lay) was a dark-coloured eschar, larger than a half-dollar, and the remainder of its surface presented a dusky, livid hue. The limb below was swelled to double its natural size. His pain was constant and at times, very severe."

The ligature of the main arterial trunk supplying the tumour was determined on, and performed while the patient was under the influence of chloroform. The preliminary steps of the operation were somewhat embarrassed by the corpulence of the patient, but the vessel was soon reached, and carefully exposed for the space of three-fourths of an inch; and being recognised to be healthy, a ligature was readily thrown around it and tied, just half an inch above the bifurcation into internal and external iliacs, with the result of at once arresting all pulsation in the tumour.

The operation, on the whole, was well borne; the shock was slight, and reaction immediate and favorable. The temperature of the limb was, for a few hours, lower than that of the other, but afterward did not vary from that of the rest of the body. The patient did well until the close of the second day, when he had a chill, succeeded by fever; and the tumour, then reduced

to one third of its former size, became exceedingly tender and painful. These symptoms continued to increase, and the strength to decline, until death ensued at the close of the fourth day.

Post-mortem examination the next day.—The tumour and surrounding parts were found to be the seat of inflammation, which had resulted in suppuration. It contained recent coagula, with some layers, slight in thickness, of older date, and involved the external iliac to within an inch of its bifurcation. Wound had united throughout, except in the track of the ligature. There was no peritonitis; death having resulted from the irritative fever, which accompanied the suppurative inflammation in and around the aneurismal sac. On laying open the arteries in the vicinity of the ligature, a firm, conical coagulum was found, both above and below the place of deligation; the latter clot being more than half an inch in length and bifurcated—one portion being prolonged into the internal and the other into the external iliac arteries.

ART. 129.—*A new method of operating in post-fascial Abscess originating in the Iliac Fossa.* By Dr. GORDON BUCK, Surgeon to the New York Hospital.

(*New York Journ. of Med.*, March, 1857.)

This paper details the minute history of seven cases of “a class of abscesses which,” says the author, “might easily be confounded with others, the prognosis of which is less favorable, and in which the same treatment might hasten the fatal termination to which they so generally tend. The particular mode of treatment adopted in these cases, it is believed, has not hitherto been generally employed. It was first suggested by a careful study of the surgical anatomy of the parts involved; and the successful results which have followed its use may claim for it a favorable consideration.”

In the account of the first case, the author's description of his method of opening the abscess is as follows: “An incision, one inch in length, was made at a finger's breadth below the middle of the outer half of Poupart's ligament through the skin, subcutaneous tissue, and fascia lata; a probe was then passed deeply upward and backward, and on being withdrawn, was followed by an escape of pus; whereupon the track of the probe was enlarged, and a free outlet afforded for the contents of the abscess, with evident relief to the patient.” In another case, after the incision “down through the fascia lata,” a grooved exploring needle was passed upward and backward to the depth of about two inches; the opening was then enlarged and the matter withdrawn. In a third case, the incision through a tense and unyielding fascia was followed by the introduction of a trochar exploring needle in a canula, up and backward into a free cavity, from which pus escaped through the canula. In a fourth case, after the incision and passing of the needle, trochar, and canula, upward and backward behind Poupart's ligament, a freer outlet was secured by the enlargement of the track, with a narrow-bladed knife passed along the side of the canula before its withdrawal, and by the subsequent temporary employment of a tent.”

Elsewhere Dr. Buck adds: “The treatment of abscesses in the iliac region, laid down by surgical authorities, does not differ materially from

that of abscesses situated elsewhere. Means suited to produce resolution of the inflammation and prevent suppuration are advised in the early stage. When these have failed, such means as will hasten the approach of matter to the surface, and thus afford an opportunity to evacuate it by an opening, are then to be resorted to. In this form of abscess, the first object would be very rarely attained, owing to the remoteness of the seat of inflammation from the surface, and its confinement beneath a strong and tense fascia. For the same reason, the approach of suppuration to the surface would be very tardy, and hence great danger might be apprehended from the injurious effects of long pent-up matter, such as caries of the os ilii, as was threatened in Case No. 3, where denuded bone was encountered at the bottom of the abscess; also, deep burrowing along the walls of the pelvic cavity and openings into the peritoneum, intestines, vagina, and bladder. With the view of averting these disastrous consequences, a point was sought where an opening might be early established for the escape of the suppuration. The point selected possesses the advantage of safety in respect to its anatomical relations, it being remote from the peritoneum and important blood-vessels; it affords, also, the most dependent outlet. The mode of performing the operation has been sufficiently described in the preceding narrative. No time should be lost in resorting to it; the absence of fluctuation need not deter from it. The phlegmonous character of the swelling; its anatomical relations to the iliac fossa and Poupart's ligament; the absence of disease of the lumbar vertebræ, and the coexisting retraction of the thigh; these points being clearly made out, are sufficient to warrant the conclusion that suppuration has taken place in the fossa behind the fascia.

"It is hardly necessary to add that the approved auxiliary means of abating inflammation, such as leeches, poultices, &c., together with appropriate constitutional remedies, are not to be omitted."

ART. 130.—*On supra-pubic puncture of the Bladder.* By M. FLEURY.

(*Gazette des Hôpitaux*, No. 59, 1858.)

A discussion upon this operation recently arose at the Paris Surgical Society, on the occasion of the presentation of a memoir by M. Fleury, in which he stated that he had often performed the operation with success, and considered it a very easy one.

M. Boinet regretted that the author had not stated whether his patients suffered consecutively from the adhesion of the bladder to the abdominal wall at the seat of puncture, and the consequent impediment to the functions of the organ.

M. Chassaignac could not admit the ill consequences said to result from these adhesions—such, indeed, not being observed in the case of the high operation for lithotomy, after which much more considerable adhesions take place. He did not, however, admit that puncture of the bladder was the simple operation it was stated to be by M. Fleury. When the abdominal parietes are very thin, and the bladder is much distended, few precautions are necessary; but in very fat or very muscular subjects, serious difficulties may arise. We have then to employ

a very long trocar, and to plunge it in very deeply ; and there is danger of wounding the opposite side of the bladder—an accident which has occurred to M. Chassaignac himself.

M. Robert considered puncture of the bladder as preferable to forced catheterism, and has had recourse to it seven or eight times. He does not fear the accidents attributed to the retention of the metallic canula in the wound. The substitution of a gum-elastic catheter for this is sometimes very difficult, owing to the contraction or deviation of the track made by the canula, and the attempts may give rise to painful laceration and to infiltration. He therefore leaves in the canula for a fortnight, and then substitutes a caoutchouc tube. He observed, also, that the urine should not be allowed to run continuously from the canula. This should be plugged, and only opened three or four hours ; otherwise the bladder, contracting too readily upon itself, may abandon the canula.

M. Deguise could not understand how any difficulty could arise in introducing a catheter by the track of a canula that had remained *in situ* for eight days. For his own part, he introduced a catheter on the first day, and changed it on the third or fourth, and he had never found any difficulty in so doing. He considered the operation a very easy one, providing that a preliminary incision be made down to the linea alba. He employs also a straight, in preference to a curved canula, the latter being liable to injure the *bas-fond* of the bladder or the prostate. The trocar is to be introduced horizontally, and a gum-elastic catheter is to be passed into the canula immediately on the withdrawal of the stiletto, and to be fixed *in situ* when the canula has been slid away upon it.

M. Huguier maintained that there were conditions which rendered this a very difficult operation, and among these is particularly the ascension of the prostate and *bas-fond* of the bladder. When the bladder becomes much distended, it rises, like the uterus in pregnancy, above the superior aperture of the pelvis ; and in complete retention, the fluctuation felt through the rectum—so much spoken of by authors—cannot be felt, because the bladder becomes raised to a point beyond the access of the finger. Under these circumstances the prostate may be wounded, and that when in nowise enlarged. This occurred to Richerand, who was a great advocate of the operation, and very skilful in its performance. To avoid this accident, the straight should be preferred to the curved trocar ; and this should be passed horizontally above the pubis, instead of, as is usually directed, downwards and backwards. He seldom removes the canula before the seventh day, and has never known a straight instrument excite any irritation of the posterior wall of the bladder. He, however, takes the precaution of introducing a gum-elastic catheter into the canula, and fixing it there ; so that its smooth, rounded extremity, furnished with its lateral eyes, may project at least a centimetre beyond the vesical extremity of the canula.

M. Giraldès could not agree with M. Huguier in admitting this ascension of the *bas-fond*, the ease with which the organ may be punctured by the rectum showing that the relation of the parts is not changed. This rectal operation has very often been performed by Mr. Cock, of Guy's Hospital, who finds it of much easier execution than the supra-pubic puncture.

M. Lenoir has been struck with the facility with which some surgeons decide upon the performance of this operation. For his own part, although attached to the hospitals for twenty years, he has never had recourse to it but twice. He thinks it should be reserved for extreme cases, when the rupture of the bladder seems imminent. Then the bladder is very distended, and its puncture presents no difficulty. He entirely rejects the opinion of M. Huguier as to the ascension of the prostate. This gland is solidly fixed, and if it sometimes extends above its ordinary level, it is only because it has become hypertrophied. It is possible, that when it is greatly hypertrophied, it might become punctured by a curved trocar; but M. Lenoir never exposes himself to such an accident, because he never punctures the bladder for prostatic retentions. Such an obstacle being permanent, there is no chance of re-establishing the natural course of the urine, and the patient is exposed to the inconvenience of a hypogastric fistula for the rest of his life. In such a case, forced catheterism, by means of a conical instrument, is far preferable. It is an intra-urethral puncture through the prostate; and a false passage, an intra-prostatic fistula, is produced that will fulfil the functions of the prostatic portion of the urethra. The operation completed, the conical catheter should be replaced by one in gum-elastic—a practice much preferable to leaving in the metallic instrument.

M. Huguier added, that although the anterior portion of the prostate is fixed by ligaments and aponeuroses, which do not allow of its rising, its posterior part is, in fact, drawn up during distension of the bladder. The finger cannot then feel the globular or cylindrical fluctuation which has been described as one of the principal signs of retention; but this does not prevent a long and concave trocar, directed backwards and upwards, from reaching the bladder.

M. Chassaignac also admits this elevation of the posterior part of the prostate. He thinks the preliminary incision of the integuments, as recommended by M. Deguise, might give rise to infiltration; and he rejects puncture by the rectum as dangerous, because of the risk of penetrating into the recto-vesical cul-de-sac of the peritoneum.

ART. 131.—*The Hypogastric Operation for Stone, aided by Cauterization.*
By M. VALETTE, Surgeon to the Charité Hospital at Lyons.

(*Lancet*, Oct. 16, 1858.)

M. Valette has recently published an essay of sixty-three pages on the above operation.

The author uses chloride of zinc paste to favour adhesion between the abdominal walls and those of the bladder. The operation consists of two stages: in the first, the bladder is reached by successive cauterizations; in the second, the extraction of the stone is effected.

The patient being narcotized, an incision about two inches long is made in the mesial line starting from the pubis; when the recti are reached they are separated with the director. A catheter, containing a stilette armed with a trocar extremity (*sonde à dard*), is then introduced into the bladder along the urethra, and the point of the stilette is made to pass out of the bladder between the recti muscles about one inch

above the symphysis. A platinum wire is then passed through an eye in the stilette, situated about three quarters of an inch from its point. The stilette is then made to re-enter the bladder, and is thrust out again two thirds of an inch lower down than the spot of its first emergence. The wire which has reappeared with the stilette is then taken out, the latter is drawn back into the bladder, and the catheter removed altogether.

At the bottom of the wound a piece of chloride of zinc paste, half an inch long and a few lines broad, is then placed; cotton wool is put over and fixed upon it by bringing together and tying the two ends of the platinum thread over it. A compress and a roller complete the dressing.

The chloride is removed in twenty-four hours; the eschar cautiously incised, taking care not to include the healthy structures beneath it, and a new piece of caustic is placed in the wound. Four or five such applications are generally necessary before the bladder is reached; oozing of urine is a sign that the eschar includes the vesical walls and that the cauterizing stage is completed.

The second stage of the operation is now entered upon by introducing the sharp stiletted catheter into the bladder along the urethra, the end of the instrument (acting then as an ordinary catheter) being made to emerge through the vesical aperture, which allows of the oozing of urine. The catheter is intended as a director for the introduction of the "lithotome caché." This latter instrument is then passed into the bladder through the above-mentioned vesical aperture, and the latter is made larger by removing the instrument, which cuts with the blade pushed out by the spring. When the lithotome caché is removed, the aperture may, if necessary, be made larger by means of a probe-pointed bistoury. Lateral incisions may even be made, if found desirable. The stone is then removed by means of the gorget and forceps.

M. Valette has operated in this manner upon four boys, and with complete success; one of the patients having, however, died some time after leaving the hospital, from adynamic intermittent, the wound being quite healed and micturition normal. The ages were respectively five, seven, eight, and ten years. The urine passed entirely through the urethra in from sixteen to twenty-one days after the operation; and the wound of the abdomen was healed, on an average, towards the thirty-fifth day. The first two patients were kept under observation nearly two months after the closure of the wound; their health improved all the while, and the vesical contractions seemed to be effected with as much vigour as before the operations. The author concludes with these sensible words: "The path is now open; it is to be hoped that other surgeons will engage in it, and that facts will soon be numerous enough to enable us to judge definitely of the value of the hypogastric operation for stone, performed with the assistance of cauterization."

ART. 132.—*On the application of Sutures to the Bladder in the High Operation for Stone.* By Dr. LOTZBECK.

(*Deutsche Klinik*, No. 15, 1858; and *Med.-Chir. Review*, Oct., 1858.)

Dr. Lotzbeck, of Tübingen, observes that although the high operation has of late been more frequently performed than formerly, it has not ob-

tained that admission into practice which its advantages might have led us to expect. Professor Günther's statistics of the high operation have sufficiently shown that the fear of urinary infiltration has been exaggerated. The object of this paper is to advocate the employment of the suture of the bladder, a practice revived of late, and opposed by Bardeleben, Pitha, and others, upon mere *à priori* reasonings. To them may well be opposed the practical experience of Professor Bruns, of Tübingen, who has employed sutures in numerous cases, without finding much difficulty in the execution of the operation or danger in the result.

A case is given in illustration. A large stone being found in the bladder of a child eleven years old, it was determined to remove it by the high operation. After the bladder had been filled with lukewarm water, a vertical incision was carried two inches above the symphysis, while two lateral incisions, half an inch long, detached the pyramidales from their insertion in the pubis. Professor Bruns has on several occasions operated by a transverse in place of a vertical incision; but when the stone is large, he finds that such incision cannot be made long enough without injuring the inner crus of the outer ring, and thereby causing a risk of future hernia. Indeed, the author knows of a case in which a double hernia followed such incision of the two *crura interna*. The opening into the bladder and removal of the stone (which weighed nine drachms and a half) were rapidly accomplished. The bladder was somewhat raised by a hooked forceps, and four points of suture were inserted into its walls (avoiding the mucous membrane) by a strongly curved needle, one end of the sutures being cut close and the other brought out externally. The keeping the external parts of the incision well apart was much facilitated by an instrument resembling the dilating forceps used by Trousseau in tracheotomy, the help of an assistant being thus dispensed with. In order to be certain that the edges of the wound in the bladder were closely applied, it was filled with water injected by the urethra, and not a drop escaped by the wound. After the operation was completed, an elastic catheter was left in the urethra, which allowed of the easy discharge of the urine. Not a drop of this fluid issued by the abdominal wound, nor of the water that was frequently injected by the urethra to cleanse out the bladder. The sutures came away easily on the sixth or seventh day, and on the tenth the catheter was entirely removed, having been occasionally so for some days previously. The urine could now be held for a long time, and discharged in a good stream. The cicatrization of the wound in the abdomen took place in the course of three weeks.

ART. 133.—*Some trials made to facilitate the removal of Stones from the Urinary Bladder.* By Dr. A. BUCHANAN.

(*Glasgow Med. Journal*, July, 1858.)

Among the advantages which attend the rectangular operation of lithotomy, one is to render more easy the extraction of the stone in the ordinary way, with the aid of the forceps. This depends upon the nature of the operation, which, by diminishing, as far as can be done, the distance between the opening made in the bladder and the external

aperture of the operation wound, brings the stone more within reach, and thus facilitates the ordinary manipulations for extraction. Of the extent of the facilities so obtained, those only can judge who are familiar with this operation, and have had opportunities of comparing it with the old lateral operation, which it has for many years past superseded in and around Glasgow—the only field on which hitherto the two operations have come into competition with each other. Still, however, even with these facilities, there is usually more delay and more difficulty experienced in laying hold of and extracting the stone, than in making the incision necessary to get at it. It is therefore chiefly to this second stage, that any attempt to simplify and improve further the operation of lithotomy should at present be directed.

Influenced by these views, Dr. A. Buchanan has been led to make trial of various methods of removing stones from the bladder; and he now proposes to bring under the notice of those who take an interest in such researches, and understand the capabilities of the new mode of operating, one or two of the methods which he tried, and thought most promising of good results:

Extrusion with the fingers.—The first method is, perhaps, more curious than useful. He found that after making the usual incision, according to the rectangular method, it was quite possible to remove certain stones with the fingers alone, without the aid of any instrument whatsoever. Stones of a spherical shape and smooth surface, like marbles, even though of large size, could readily be so removed; but to stones of a different shape, and of rougher surface, this method was found to be inapplicable, unless the opening in the bladder were of large size. The following remarks will render the mode of manipulating easily understood:

The forefinger of the right hand, introduced into the bladder through the operation wound, readily reaches the stone, and has it so much under command, that it can easily be brought down into the triangular space at the neck of the bladder, and placed there so that its longest diameter—if it be not spherical—may be parallel to a line at right angles to the middle of the incision of the bladder. This is the position most favorable for the extraction of the stone, and by the pressure of the forefinger it can be retained steadily in that position. Holding it, therefore, firmly, the fore and middle fingers of the left hand are introduced into the rectum, and passed up beyond the prostate, when, upon pressing them forwards, the stone is distinctly felt by them; and it is so firmly grasped between these two fingers and the forefinger of the right hand, as irresistibly to suggest to the mind the attempt to extrude it from the bladder, by means of the two fingers of the left hand pressing it from behind, while the forefinger of the right hand guides it outward, and regulates the direction of the pressure.

In this way, as stated above, smooth and spherical stones are readily removed, but only these, unless with a larger incision than usual. The process is, therefore, of such limited utility, that it would not have been worthy of the space the description occupies, had it not been that to bring the stone and fingers into the positions just described is often an important preliminary to other methods of extraction;—as to that next to be described, and to the ordinary method of extraction with the

forceps—if it be the latter, instead of grasping through the bladder to find the stone, and attempting to seize it in whatever position it lies, the forceps is at once laid over the stone in a known position, and the stone is thrust between the blades by the fingers of the left hand, and laid hold of in the most advantageous direction; that is, with its long diameter parallel to the blades.

Landing-net.—The great majority of stones cannot be extracted by the fingers alone, but require the aid of an instrument. Of various instruments for the purpose which Dr. Buchanan had constructed and tried, the one which seemed to me to answer best the ends for which it was devised, he named a *landing-net*, from its resembling very much, in its mode of operation, the tackling of the same name, with which the angler secures the fish he has hooked and brought to the water's edge. He had this instrument first made with a single handle, and an oval elastic rim of whalebone, to which the net was attached. But finding this not to answer well, he returned to a construction differing only from that of the common forceps, furnished with a net, in the modifications to which he subjected it. The handles are in no respect different, but the blades have altogether changed their character, as they are no longer intended to grasp the stone, but merely to open and shut the mouth of the sac which is attached to them. In conformity with this new destination they are rounded and attenuated, so as to resemble stout stocking-wires. They are curved, so as to form together, when closed, an oval orifice to the sac; and they terminate in two rounded knobs, like peas, which prevent them from doing any injury when they are introduced into the bladder, or when opened and shut within it.

An instrument of this kind lays hold with great facility of stones of the size of those usually met with in the bladder. On placing the mouth of the instrument over the stone, and making downward pressure, the mouth tends to open spontaneously to admit the stone: and next, the hand holding the instrument, which had at first yielded to, and gently assisted the distending force, now closes the mouth, so as to include and secure the stone within the sac. In this way, without any further preliminaries, a stone may be laid hold of within the bladder, the metallic knobs detecting the place of the stone; and these knobs being carried to the further side of it, the mouth of the sac lies over the stone, and, on downward pressure being made, will open to receive it. This method, however, is far inferior in precision and facility of execution to the following method, which he recommends in preference:

Place the stone and the fingers in the positions recommended for the process first described; that is, the stone lying immediately behind the opening in the bladder, with its long diameter at right angles to the direction of the opening; the index finger of the right hand introduced through the wound, and resting on the stone, and the fore and middle fingers of the left hand in the rectum, ready to press upon the stone from behind. Let the finger of the right hand be now withdrawn, and the instrument introduced in its place. The stone is now pressed by the fingers of the left hand against the two wires forming the mouth of the sac, and there separating the stone is forced into the sac itself, and secured by the shutting of its mouth. The two slender wires of the

sac add little to the bulk of the stone, so that any difficulty experienced in extracting it can depend only on a disproportion between the size of the wound and the stone which is to pass through it; and if that cannot be overcome by address and moderate traction, it must be met by the enlargement of the wound.

ART. 134.—*A Statistical Report of Forty-six Operations for Stone in the Bladder, with the best method of performing Lithotomy.* By Dr. PAUL F. EVE.

(*Nashville Journal of Med. and Surg.*, Aug., 1857; and *North American Medico-Chirurgical Review*, July, 1858.)

The first part of this paper is occupied with a synopsis of twenty-one cases, in continuation of a report of twenty-five similar cases which was published in the April number of the 'Am. Jour. of Med. Sciences' for 1852.

"Since October, 1841, the date of my first operation, a period of a little over fifteen years, I have operated on forty-six cases of urinary calculi, thirty-eight of them under chloroform, without an unpleasant result from it, and have removed one hundred and sixty-six stones. The following tables will present the statistics of these cases in regard to the date, age, sex, race, residence, method of operation, number of calculi, and the result of each one.

Statistics of Forty-six Operations for Urinary Calculus.

No.	Date.	Age.	Sex.	Race.	State.	Operation.	No. of calculi.	Result.
1	1841	8	Male.	Mulatto.	Ga.	Bi-lateral.	1	Speedy recovery.
2	1843	6	"	White.	Ga.	"	1	" "
3	1843	3	"	Mulatto.	Ga.	"	1	" "
4	1845	3	"	White.	Ga.	"	1	" "
5	1845	34	"	"	Ga.	Lithotrity.	1	Recovery.
6	1846	24	Female.	Black.	Ala.	Vaginal section.	1	Speedy recovery.
7	1847	20	Male.	White.	Ga.	Bi-lateral.	1	" "
8	1847	20	"	"	Ga.	"	1	" "
9	1848	5	"	"	S. C.	"	1	" "
10	1848	5 $\frac{1}{2}$	"	"	S. C.	"	1	" "
11	1849	50	"	"	S. C.	"	117	" "
12	1849	6	"	Black.	S. C.	"	1	" "
13	1849	4	"	White.	Ga.	"	1	" [nearly healed.
14	1849	10	"	"	Ga.	"	1	Died of dysentery when wound
15	1850	12	"	"	Ga.	"	None.	Speedy recovery.
16	1850	7	"	"	Ga.	"	1	" "
17	1851	4	"	"	S. C.	"	1	" "
18	1851	7	"	"	Ga.	"	1	" "
19	1851	7	"	"	Ga.	"	2	" "
20	1851	77	"	"	Ga.	"	3	Death in sixty hours.
21	1851	24	"	"	Tenn.	"	1	Speedy recovery.
22	1851	5	Female.	"	Ky.	"	1	" "
23	1852	7	Male.	"	Tenn.	"	1	" "
24	1852	24	"	"	Tenn.	"	1	Death sixth day.
25	1852	65	"	"	Tenn.	"	1	Death about thirteenth day.
26	1853	3	"	"	Tenn.	"	1	Speedy recovery.
27	1853	4	"	"	Tenn.	"	1	" "
28	1853	10	"	"	Ga.	"	1	Recovery.
29	1853	20	"	"	N. York.	Lithotrity.	1	Speedy recovery.
30	1854	34	"	"	Ga.	Bi-lateral.	2	Recovery.
31	1854	12	"	"	Tenn.	"	1	Speedy recovery.
32	1854	7	"	"	Tenn.	"	1	" "
33	1854	23	"	"	Ga.	"	1	" "
34	1854	26	"	"	Ga.	"	1	Recovery.
35	1855	5	"	"	Tenn.	Lateral.	1	Speedy recovery.
36	1855	16	"	Mulatto.	S. C.	Bi-lateral.	1	" "
37	1855	9	"	White.	Tenn.	"	1	" "
38	1855	66	"	"	Miss.	High operation.	1	" "
39	1856	2 $\frac{1}{2}$	"	"	Tenn.	Bi-lateral.	2	" "
40	1856	11	"	"	Tenn.	"	1	" "
41	1857	26	"	"	Tenn.	Dilatation.	1	" "
42	1857	22	"	"	Tenn.	Bi-lateral.	1	Slow recovery.
43	1857	19	"	"	Tenn.	"	1	Speedy recovery.
44	1857	8	"	Black.	Tenn.	"	1	" "
45	1857	61	"	White.	Tenn.	"	1	" "
46	1857	9	"	"	Tenn.	"	1	" "

"Rejecting the case of death from prevalent dysentery when the wound was nearly healed, the mortality exhibited by these statistics is 1 in 15 $\frac{1}{3}$, and 35 of 46 had a speedy recovery."

Summary of the Forty-six Cases.

No. of Cases.	Age.	Sex.	Race.	Operation.	No. of Stones.	Result.
	27 under 15. 15 adults. 1 aged 61. 1 " 65. 1 " 66. 1 " 77.	44 males. 2 females.	40 whites. 3 mulattoes. 3 blacks.*	40 bi-lateral. 1 lateral. 1 vaginal. 1 high. 1 dilatation. 2 lithotrixy.	117 in one. 3 in one. 6 in three. 2 each. † 40 in 41.	Death occurred from dysentery in one of the 27 under 15. Death in one of the 15 adults. Death in 2 of aged. 36 had a speedy recovery.
46	46	46	46	46	166	

Dr. Eve informs us that he "had not declined to operate in a single case under his care where, in the opinion of others, it ought to have been done. In one instance, a patient died a few days after being placed on the table, though no operation was attempted, and two others expired after leaving home to seek relief. I know now that I operated on two cases which were at the time beyond all expectation of a cure, and another one, it is possible, might have been relieved by lithotrixy or the high operation.

"As to the best method of performing lithotomy, it is very certain, as in many other operations, that no one in particular should be adopted to the exclusion of all others. Each case is a problem to be solved best by a careful study, and the adaptation of therapeutic means to meet all its peculiarities. While perfection may not be attained by any plan yet proposed for removing stone from the bladder, that operation attended with the least risk to the patient and followed most generally with success, is the one to be selected.

"I acknowledge, in the child or boy up to about fifteen years of age, that an experienced surgeon may, with the scalpel alone, make a capital operation and obtain good results; but in a majority of cases, and under ordinary circumstances, I have little doubt, when the method about to be described is understood, it will be admitted to be the best. My staff has, at the upper part of its groove, an opening large enough to admit the beak of the lithotome, and which is there so contracted as to prevent the escape of the cutting instrument until it arrives near the extremity, and has consequently entered the bladder. It acts, in other words, as a safe and certain director to the lithotome. It guides it with unerring precision into the bladder, and thus prevents the rectum being wounded, or the cutting instrument passing into the space between these two organs,—an event which has too often happened.

"I have also added a slight modification to the double lithotome cachée. I found the shoulder of it too large, especially for children, to enter

* I have had only one coloured patient of the West.

† In one case I failed to remove the stone. This was in 1850, and two months ago the patient arrived here, seeking relief from symptoms of urinary calculus, under which he has laboured ever since I operated upon him. He is a member of the class in the literary department of our university.

freely upon the staff when in the urethra. To prevent laceration of this canal I have had two small blades put upon the shoulders, so that the instrument may cut its way into the bladder.

"In operating for urinary calculus, I make an inverted λ incision in the perineum; thus, with a scalpel, beginning at the bulb of the urethra, an incision three quarters of an inch in length is made to the median line, then the knife turned to the left, to terminate about midway between the anus and tuberosity of the ischium. A similar leg to the inverted λ is then made on the other side, but with its cutting edge turned up, and arriving at the median line, the point of the scalpel is entered down to the staff in the urethra, upon which the lithotome is now conducted into the bladder. The staff being removed, and the half-turn given to the lithotome, the blades are expanded and the bi-lateral section made in withdrawing this instrument, which will be found to correspond very nearly with the external inverted λ incision previously made in the perinæum.

"The advantage of this external incision over the crescentic one of Dupuytren is, the facility with which it may be made and the urethra opened upon the staff. I make as small an opening in the prostate gland as will permit the extraction of the stone, knowing from experience the parts may be considerably dilated by gentle persevering traction with the forceps grasping the calculus. And the only unpleasant occurrence ever met with in the treatment of cases thus operated upon, placed under my care, has been hæmorrhage; and how this may be arrested I have described. The development of the vessels about the neck of the bladder and perineum in calculous patients, and the irritation created by the pressure of urine over the wounded surface, will account for the special and unavoidable liability to bleeding after lithotomy."

ART. 135.—*Case of Calculus in the Bladder, in which there was a communication between the Bladder and Intestine.* By Mr. CHARLES HAWKINS, Inspector of Anatomy, &c.

(*Proceedings of the Royal Med. and Chir. Society, June 22, 1858.*)

CASE.—In July, 1857, a gentleman, fifty-five years of age, consulted the author suffering with all the symptoms of stone in the bladder in a most aggravated form, the urine being alkaline, and depositing much ropy mucus. He gave the following history of his case.

"In February, 1855, I first discovered that I passed in my water a substance having a most offensive smell. My medical attendant came to the conclusion that it was fæcal matter, and that a communication existed between the bladder and bowel. During the year 1856 I ceased to pass any of this substance, but symptoms of disease in the bladder set in, and continued during the whole of the year. In the beginning of the year 1857 these symptoms much increased, and my general health became much impaired."

Notwithstanding the history of the case and the state of the bladder, the author determined to attempt to remove the stone by lithotripsy, as the patient was sinking from his severe sufferings. The first and second time of operating the patient was placed under the influence of chloroform (not so on future occasions, as he bore the operation remarkably well without it). The

bladder held sufficient water, and a very large stone was readily seized and crushed.

On the day following the fourth operation, which took place three weeks after the first, the patient had an attack of retention of urine, which lasted some hours (of which the author was not informed), when suddenly he passed a large quantity of urine and blood through the rectum. It was then evident that the opening which had formerly existed between the bladder and intestine had given way, although no opening could be detected by the finger on this or any other occasion. In a few days, having rallied from this accident, he left London, his general health being very much improved. He returned to London in October, and although the opening still existed, the bladder retained six or eight ounces of water. After two operations, which gave him no inconvenience, he went home.

In January, 1858, symptoms of stone reappearing, the bladder was examined; a small stone found and crushed. In a fortnight he returned home quite well, and has continued so ever since; but passing some fæcal matter with his urine from time to time, he was directed to wash out his bladder with warm water daily, to prevent any accumulation.

The stone was composed of the triple phosphate, having some vegetable matter as a nucleus.

The interest of this case depends on the circumstance of the communication between the bladder and intestine having taken place without causing the patient any inconvenience, until the fæces were detected in the urine; the successful issue of lithotrity where the stone was so large, the bladder in such an unhealthy condition, and a communication existing between it and the *intestines*.

ART. 136.—*Lithotritic instruments in cases of enlarged Prostate.*

By Mr. COULSON, Surgeon to St. Mary's Hospital.

(*Lancet*, Jan. 30, 1858.)

In an interesting paper, Mr. Coulson explains the necessity which exists for employing a peculiar lithotrite in patients who have stone and enlargement of the prostate.

"The changes," he remarks, "in the genito-urinary organs produced by enlargement of the prostate, and requiring the use of peculiar lithotritic instruments, may be explained in a few words. Many men who have passed the middle period of life, labour under some enlargement of the prostate, and such a condition of the gland seriously interferes with the functions of the bladder; but, for my present purpose, it will suffice to consider the effects produced on the urethra and floor of the bladder by any considerable swelling of the prostate. A consideration of these effects will at once show in what manner the modified instruments that I employ in such cases are, of necessity, required. A constant effect of prostatic enlargement is elongation of the urethra. This lengthening of the canal may be connected with several conditions of the enlarged gland; it is, however, mainly confined to the prostatic portion of the urethra, and occurs in one of two ways. As the enlargement pushes up the neck of the bladder under the arch of the pubes, it necessarily draws up and elongates the vesical end of the urethra; or, while the gland is slowly enlarging from before backwards, that portion of the urethra which traverses it must necessarily follow the abnormal development of

the prostate, and become elongated. In his recent work on the prostate,* Mr. Thompson observes, 'that in some preparations which he examined, the urethra measured three inches from the orifice of the bladder to the membranous portion, instead of an inch and a half, which is the normal length.' In all cases of this kind, more especially when the middle lobe is chiefly affected, the orifice of the bladder is thrown backwards in proportion to the development of the enlarged lobe; and hence the point of any instrument used is apt to catch against the superior wall of the canal before it enters the bladder. This is an obstacle which the surgeon is very likely to meet with; and I may add, the shorter the instrument he employs, the more likely is the obstacle to occur.

"The effects of the morbid growth on the cavity of the bladder also require some notice. These effects, so far as regards my present subject, will depend on the degree of prostatic enlargement, and on the lobe principally affected. When the enlargement is chiefly confined to the middle lobe of the prostate, which encroaches on the floor of the bladder, the capacity of that viscus at its lower part is proportionably diminished. I have seen a considerable part of the bladder occupied by the enlarged prostate; and, in extreme cases, preparations of which are preserved in our museums, the morbidly developed gland has occupied a great portion of the vesical cavity. In all the cases now alluded to, the effect of this extension of the middle lobe of the prostate backwards is to form a reservoir or sac behind the enlarged gland. The floor of the bladder is here greatly depressed; the urine remains as in a sac; and here a calculus may be lodged, the detection of which is often extremely difficult. I have, at the present time, under treatment a patient who has a stone concealed behind an enlarged prostate, the existence of which had escaped the notice of those surgeons by whom he had been previously examined.

"The necessity of employing special sounds and catheters in cases of enlarged prostate is well known to all practitioners. The increased length of the urethra, and the encroachment of the enlarged lobe on the floor of the bladder, compel the surgeon to use a much longer catheter than usual. In prostatic enlargement, the ordinary catheter will not penetrate into the bladder; and the experienced practitioner at once suspects the existence of the complaint from this circumstance. To enter the cavity of the bladder, he takes a catheter from two to four inches longer than the one in ordinary use; and with an instrument of this kind, he succeeds, after having passed it about twelve inches beyond the orifice of the urethra. The same holds good with regard to the sound. Thus the stem of the ordinary sound measures seven inches and a half; the stem of a moderately sized prostatic sound measures nine inches and a half. An ordinary catheter now before me measures nine inches and three quarters to the point. A prostatic catheter measures thirteen inches and a half. From fourteen to sixteen inches is the length recommended in standard works.

"In cases of enlarged prostate, then, the surgeon requires a long catheter of peculiar shape to draw off the urine. For the same reason—viz., the increased length of the urethra, and the depression in the

* 'The Enlarged Prostate,' p. 23, 1858.

floor of the bladder—he will require a long lithotrite of peculiar shape, in order to catch and crush the stone with ease to himself and safety to his patient.

“If the long prostatic catheter be expedient, the long prostatic lithotrite is, *à fortiori*, indispensable. The necessity of employing a longer instrument than usual in such cases will, I imagine, be generally admitted; and if I insist on it here, it is because our standard works do not allude to the point—an omission of which I have been no less guilty than others, and which I would now repair. The ordinary lithotrite of Charrière measures ten inches from the root of the stem to the eye; the chord of the curve, from the eye to the point, is one inch and three eighths. The long lithotrite, manufactured expressly for me by Charrière, measures twelve inches in the stem, and one inch and three eighths in the curve. The stem of Weiss’s ordinary lithotrite measures nine inches; the stem of the long instrument which he has made for me measures ten inches and a half; the beak is the same length in both, about an inch and a half.

“I need hardly occupy much space in dwelling on the necessity of the surgeon being provided with a long instrument of this kind, and of the many advantages which he will derive from its use. I can only say that I have had cases which, I feel convinced, I could never have conducted to a successful termination without it. It should be remembered, that with an elongated urethra, the distance between the external and internal orifices of the canal is increased by at least an inch. The enlarged prostate, again, occupies the front part of the floor of the bladder, on which it encroaches another inch or more. The calculus lies concealed in a sort of pouch behind the enlarged lobe, which rises like a barrier before it. Under these circumstances, it is evident that the surgeon will require an instrument longer than that in ordinary use, by two or three inches. The common lithotrite will either not pass into the cavity of the bladder, or, if it does, after having been forced up to the handle, the motion of its curved part will still be greatly impeded by the prostate.

“In some of my cases the common instruments were not sufficiently long, and it became necessary to push them up to the shoulder before I could turn the point in the necessary direction. With the long lithotrite, on the other hand, the surgeon gets readily into the bladder; but to overcome the impediments likely to arise from an enlarged middle lobe, a peculiarly formed beak is necessary, and the pelvis must be raised.

“The beak or curved part of the lithotrite must be short, and the curve sharp. With an instrument of this kind, the surgeon will often be able to ‘fish up the stone from the depression behind the enlarged prostate,’ in the manner described in the last edition of my work on the bladder.* By turning the point down, and elevating the handle of the instrument, the stone will commonly be found in the position already mentioned. When the middle lobe of the prostate is much enlarged, and extends some way into the bladder, the point of the instrument cannot be turned

* ‘The Diseases of the Bladder and Prostate Gland,’ p. 463. Fifth Edition. London, 1857.

downwards in such a way as to reach the stone. The plan from which I have derived most benefit in such cases is that of raising the pelvis of the patient in such a way that the calculus shall be displaced towards the posterior wall of the bladder. Especial care must be taken that the pelvis itself is raised, and not merely the lower extremities. By adopting this plan, I have, on several occasions, immediately caught the calculi, which lay concealed behind the prostate as long as the patient retained the ordinary position. Every lithotrity couch should be provided with some mechanical contrivance for executing this proceeding quickly. The sufferings occasioned by the attempt to seize a stone behind an enlarged prostate, in the ordinary way, and after strong elevation of the handle, are often of the most distressing kind, and cannot certainly contribute to the well-doing of the patient. On the other hand, the ease with which the reversed beak falls on the stone is remarkable; but the manipulation requires a cautious and practised hand."

ART. 137.—*Case of fibrous Polypus of the urinary Bladder, with remarks, &c.* By Mr. BIRKETT, Surgeon to Guy's Hospital.

(*Proceedings of the Royal Med. and Chir. Society*, May 25, 1858.)

The new growths developed in the bladder may be thus described :

1. Papilloma.
2. Fibrous polypus.
3. Villous growths.
4. Epithelioma.
5. Carcinoma: *a*, infiltrating; *b*, tuberos.

It was upon the second class that the author desired to concentrate attention. This growth resembles in every anatomical particular the "nasal polypus," which is the true type of the disease. These growths occur very rarely, and the author can find only ten cases, the description of which accords with that about to be given. Warner, Baillie, Walter, Crosse, Savory, Chopart, and Petit, each relate a case; and to these three others are added. A girl, æt. 5, came under the author's care in Guy's Hospital, in December, 1857. She was in a most cachectic state; and her mother said she had been ill about two months. At the time she complained of pain in the hypogastric region, which was soon followed by dysuria, and at last retention of urine. A catheter was used to relieve this difficulty. When admitted the urine dribbled away; but unless a catheter was introduced, the bladder became immediately distended. The urine was ammoniacal, contained mucus and blood, and was very offensive. The constitutional powers of the patient were much reduced; but after nutritious diet and tonics, the child's powers rallied, and she became a little stronger. In order to examine the bladder with more care, the patient was placed under the influence of chloroform, and then the contracted bladder was discovered to be filled with a solid growth, a portion of which had dilated the urethra, and protruded between the vulva. A ligature was applied around the pedicle of the protruding portion, which eventually sloughed off. Some days after

this the patient became very ill, and died on the twenty-fifth day after admission.

Necropsy.—Suppuration in right kidney, dilated ureters and pyelitis. The walls of the urinary bladder were hypertrophied, and its cavity was dilated. Attached to the interior wall of the viscus and the meatus, were pedunculated growths, which had dilated the meatus, and protruded through it. These consisted of delicate fibre-tissue covered with epithelium.

The progressive stages of the disease were next alluded to, and the cause of death in this, as well as in other cases of a like nature, traced to irritation excited along the track of the urinary mucous membranes.

Remarks were then made upon the differential diagnosis of this disease, and those known by the name of "vascular fungus," "epithelioma," and "medullary cancer;" and it was observed that, while in polypus little or no hæmorrhage occurred, bleeding, on the contrary, was diagnostic of the other growths. From calculus these growths might be distinguished by the absence of sound. A foreign body might be detected in the bladder by the sense of touch, but it could not be sounded. In reference to treatment, the morbid specimens demonstrate that very little hope can be entertained of doing good by surgical interference; but when practicable, a ligature might be safely placed around the pedicle of the growth.

The paper was illustrated by drawings of the morbid specimen in its fresh state, as well as by two preparations of the disease itself.

ART. 138.—*A simple palliative remedy for Varicocele.* By M. NÉLATON.

(*Journal of Pract. Med. and Surgery*; and *Dublin Hospital Gazette*, July 15, 1858.)

The radical cure of varicocele has been much spoken of, and the various operations recommended for this end have naturally been grounded on motives borrowed either from the causes or from the consequences of the disease. According to M. Nélaton, experience has not in any way sanctioned these motives; and it is time to put an end to the merely theoretical inventions brought forward on the subject of this malady.

It is unnecessary to follow the professor in his strictures on the opinions expressed on the supposed causes of varicocele. The chief point to be borne in mind on this subject is, that the varicose condition of the veins of the funis coincides with the period of life during which the function of generation is in possession of its greatest activity. Varicocele is a complaint special to the second epoch of youth, and tends to decrease in proportion as the subject advances in years.

M. Nélaton being intrusted with the examination of the youths destined to the imperial military schools, found that one or two in every fifty were affected with varicocele. On the other hand, while he was at Bicêtre, out of a population of 5000 old men, the same surgeon met with scarcely a single case of the same complaint. This fact should not be forgotten.

Another question is deserving of the most serious attention: it is the influence supposed to be exercised by the varicocele upon the ana-

tomical structure and functions of the testis. The testicle corresponding to the varicocele is said to decrease in size and become atrophied. Wishing to ascertain the truth of this assertion, M. Nélaton examined a large number of cases of varicocele, and recognised that, in point of fact, the testis was frequently smaller on the diseased than on the healthy side; but he obtained no proof whatever that such difference in volume was the consequence of the malady. Very few men with healthy organs present perfect uniformity on both sides; it is, therefore, highly probable that in subjects affected with varicocele, the difference of size may have been congenital. The question of mere volume is, besides, of very secondary importance to the patients; that which interests them more directly is, the preservation or diminution of the genital powers. Whether such individuals preserve their local faculties is a point which has not been inquired into, and which it is important to investigate. On examining the many subjects who have sought his advice for varicocele, M. Nélaton has acquired the conviction that the disease in no wise impairs the power of reproduction; and that if the testicle has decreased in volume, such decrease must have in all cases been very limited, the function having suffered no diminution in its activity.

Hence the alleged atrophy of the testis does not constitute, for M. Nélaton, a sufficient motive to justify any operation. He requires other reasons to resolve upon adopting such a course. The excessive volume of the varicocele is not one of these. Numberless patients are met with, who, notwithstanding the enormous magnitude of the scrotum, take, without much inconvenience, the most violent exercise. Others, on the contrary, suffer extremely from very small varicoceles. It is the presence of tenderness and of pain, carried to such a degree as to interfere with professional avocations, which must be inquired into, whenever the expediency of an operation for the cure of the disease has to be discussed.

With this exception of pain, and of the serious interference of the disease with the daily occupations of the patient, which exclusively points to the necessity of surgical action, M. Nélaton refrains from operation; and his reserve is justified by the daily observation of facts which show, as we have above stated, that varicocele is a malady belonging to youth, which has a natural tendency to disappear with advancing years. At this very moment a case in illustration of this singular phenomenon may be observed in the wards. A man suffering from a cancrroid affection of the bladder was admitted into the hospital. At the same time he was found to bear a varicocele, which has now lasted eight or ten years. It is neither painful nor troublesome; every year its size decreases, and it will, in all probability, soon disappear altogether.

Such cases distinctly point out what should be the practitioner's conduct. It is clear that he must wait, and be satisfied with the use of such palliatives as may diminish the discomfort attendant upon the complaint.

Two years ago, a young man with varicocele entered this hospital. Three years previously pain was complained of in the left inguinal ring; shortly afterwards the scrotum swelled on the same side, and became

very tender. A varicocele was recognised, and the patient was operated on at Vitry-le-Français, with Breschet's forceps, to which was added the loop of M. Regnaut, of Toulon. Notwithstanding the ability of the operator, and the care bestowed upon the case, the cure was not permanent, and after a lapse of two years this individual presented himself to M. Nélaton in the following condition: The left side of the scrotum, much increased in size, was the seat of venous enlargement. A pediculated hernia, distinct from the varicocele, existed on the same side. The hernia was reduced, and, as a remedial agent of a palliative description, M. Nélaton applied to the varicocele M. Richard du Cantal's caoutchouc ring.

This apparatus is the inoffensive realisation of Sir A. Cooper's idea. The testis and the venous chord being pushed back towards the ring, the dependent portion of the scrotum is enclosed in an elastic belt, which surrounds it like a bracelet. For this purpose a strip of common caoutchouc is prepared, about an inch in width, and of variable length, according to each case. The extremities of this band are cut with scissors, and after having been rolled round the loose scrotum, its ends are made to adhere to each other. This little apparatus, which M. du Cantal invented for his own use, has been employed by M. Nélaton in several instances, with complete success. It permits the patient to stand, walk, and even exercise violently; its application is perfectly easy; it may therefore be looked upon as a most important palliative agent, which allows the patient to gain time; and even in the worst cases it will always be well to give it a trial, before exposing the subject to the chances of an operation, which, whatever may be the method adopted, is never entirely devoid of peril.

(C) CONCERNING THE UPPER EXTREMITY.

ART. 139.—*A new principle of Diagnosis in Dislocation of the Shoulder-joint.* By Dr. L. A. DUGAS, Professor of Surgery in the Medical College of Georgia, U.S.

(*Southern Med. and Surg. Journal*, May, 1858.)

Dr. Dugas' new principle of diagnosis may be stated in the following words:

If the fingers of the injured limb can be placed by the patient or by the surgeon upon the sound shoulder, while the elbow touches the thorax, *there can be no dislocation*; and if this cannot be done, there *must be* a dislocation. In other words, it is *physically impossible* to bring the elbow in contact with the sternum or front of the thorax if there be a dislocation; and the inability to do this is *proof positive* of the existence of dislocation, inasmuch as no other injury of the shoulder-joint can cause this inability.

In order to make these propositions apparent, Dr. Dugas gives drawings taken from the skeleton, showing the relative position of the bones in the natural state, and the several dislocations of the shoulder,

and adds, that the evidence thus obtained in support of his principle, would be still stronger if the bones were invested with their normal coverings and attachments.

ART. 140.—*A Case of Dislocation of the Shoulder upwards and inwards.*
By Mr. T. HOLMES, Curator of St. George's Hospital Museum.

(*Proceedings of Royal Medical and Chir. Soc.*, June 22, 1858.)

CASE.—The patient was admitted into St. George's Hospital, under Mr. Tatum. The accident proved fatal in consequence of other very severe injuries. On examination, the head of the humerus was found immediately under the skin, having passed through the fibres of the deltoid muscle, and having the cephalic vein on its inner side. It had fractured the coracoid process in its passage upwards, and was resting behind on the stump of this process and on the clavicle, with a small portion of the coraco-acromial ligament, which remained untorn. *Internal* to it (besides the fibres of the deltoid and the cephalic vein) was found the fractured extremity of the coracoid process, with the muscles attached to it. *External*, and somewhat posterior to it, was the acromion process, separated from it by some of the fibres of the deltoid. *Below*, and a little external to it, was the glenoid cavity, the tip of which lay on a horizontal plane quite below the level of the dislocated head of the bone. The long tendon of the biceps remained still attached to the scapula, and was therefore situated below and external to the head of the humerus. The bone, in passing out of the glenoid cavity, had injured this tendon slightly, so that some of its internal fibres had been broken away from the muscle. The sub-scapularis muscle was intact. The muscles attached to the greater tuberosity of the humerus were torn through, except that a portion of the teres minor remained. The capsular ligament had been lacerated at its upper and inner part, forming a large hole for the passage of the head of the humerus. The author referred to cases somewhat resembling this, related by M. Malgaigne, in his large work, and by M. Soden, in the 'Transactions' of this Society; and concluded with some remarks upon the diagnosis and mode of treatment of this accident.

ART. 141.—*Excision of the lower four-fifths of the Radius.*
By Dr. CARNOCHAN, Surgeon to the State Hospital, New York.

(*American Quarterly Journal of Med.*, July, 1858.)

CASE.—C. W—, æt. 31, married, a native of Wales, and resident of the United States for the last twelve years, was attacked about five years since with inflammatory rheumatism, from which she suffered severely for about four months, although judiciously treated by her attending physician. Upon the subsidence of the rheumatic attack, her general health became tolerably good, but the left arm continued to be subject at times to severe pains, especially about the region of the forearm. In January, 1846, the left hand and arm became severely affected, swelling to twice the size of the sound limb, as high as the humero-scapular articulation. The disease continued unabated, accompanied with severe and excruciating pain, until July following, when signs of suppuration at the lower part of the forearm began to be manifested. The pain now became much less, and the tumefaction of the upper arm subsided: the febrile condition, also, became less; the patient,

however, at this time, from the prolonged suffering she had undergone, and the severe treatment to which she had been subjected, presented a strumous appearance, with much debility and emaciation. The local disease had become concentrated upon the forearm, and an abscess, slow in its formation, finally pointed opposite the lower portion of the radius, on the posterior aspect of the arm, about three inches above the styloid process. Another similar point of fluctuation could be distinguished in front of the radius, nearly on the same level. At this stage of the malady the patient was brought to me for my advice, by Dr. Dougherty, of Brooklyn, on the 15th of March, 1857. The forearm was still generally much swollen, tense, hard, and red, as far as the humero-cubital articulation. A bistoury was passed into the abscess on the dorsal aspect of the arm, and a considerable quantity of pus immediately escaped; a probe passed through the wound struck upon denuded bone, and the nature of the case was plainly revealed. From the general character of the swelling at the lower part of the arm, it was not easy to determine at once whether the ulna was implicated or not; it was finally concluded, however, that the radius alone was diseased from ositic inflammation and its consequences, and that resection afforded the only chance of recovery. The nature of the disease was explained to the patient, and she readily assented to an operation.

The operation was performed on April 9th, 1857, with the assistance of Drs. Casseday, Dougherty, Cummings, Abrahams, Henry, and others, mostly in the same manner as had been observed in the case of exsection of the entire radius, which has already been reported. The patient was seated upon a chair beside the operating table, and was easily put under the influence of chloroform. The arm, supported by assistants, was placed so that the ulnar border of the forearm rested firmly upon the edge of the table, in a state of semi-pronation. A longitudinal incision was made in the direction of the radius, on its external anterior border, commencing about two inches below the head of the bone, and extending downwards to a point opposite, and a little behind, the styloid process. Two terminal incisions were then made at the extremities of the first one, extending transversely backwards, about three-quarters of an inch. The bone was laid bare a short distance below its middle, and the dissection carried upwards to within three-quarters of an inch below the bicipital tuberosity, so as to expose the bone on its different aspects; at this part the radius was perfectly sound, and its section was easily effected by means of the chain-saw. The remaining steps of the operation consisted in separating the diseased portion from the soft parts, and in isolating the lower part of the radius from its attachments at the radio-carpal articulation, without injury to the arteries, nerves, or tendons. The humoral artery was compressed, during the dissection, by an assistant; the interosseous artery and a small branch only requiring the ligature. As soon as the oozing of blood had stopped, the tendons of the wrist were arranged in their relative positions, and the lips of the wound were drawn together. The proper dressings were then applied, and the forearm, in a state of pronation, was laid upon a padded splint; secured in that position by slips of a retentive bandage.

Six hours after the operation the patient was seen; she complained of much pain, extending to the axilla; an anodyne draught was ordered.

April 10th (the following day).—The patient stated that she had slept well during the night; pulse 120; ordered an anodyne for the night.

May 14th.—Six weeks after the operation, the patient is entirely well, and the wound healed, with the exception of a small point at the middle of the arm.

At the beginning of this month (June, 1858), the patient presented herself

to me, in order to express her thanks at the result of the operation, and to fulfil a promise, which I had obtained from her, of seeing me again at the expiration of a year. The radial aspect of the forearm presents the long cicatrix of the incision, and a depression corresponding to the portion of bone removed. The axis of the hand is somewhat changed, the hand being drawn towards the radial side of the arm; and, on the ulnar side of the wrist, the styloid process of the ulna presents a considerable projection. The functions of the hand and its sensibility are not impaired, and the patient informs me that she continues to perform her household duties nearly as well, and with as much facility, as before the operation.

Pathological condition of the bone.—The specimen exhibits the results of severe inflammatory action of the osseous tissue—expansion of the osseous tissue, ulceration, acicular projections, enlarged foramina with mammillated eminences, eburnation, and cloacæ, leading to dead portions of bone in the interior. The surface preserves the general outline of the bone, but by the increase of bony matter it has been thrown into ridges and tuberosities, partly by the action of the muscles of the forearm.

(D) CONCERNING THE INFERIOR EXTREMITY.

ART. 142.—*Case in which the hip-joint was excised for Morbus Coxarius, and a summary of similar cases.* By Dr. R. A. KINLOCH, Surgeon to the Roper Hospital, &c.

(*Charleston Med. Journal and Review*, May, 1857.)

CASE.—John M—, a native of Ireland, æt. 20, a labourer by occupation, was admitted into the Roper Hospital on the 2d of April, 1856. Two years previous he began to suffer from vague undefined pain about the lumbar region and the hip-joint of the right side. From time to time he had been compelled to lie up for a short period, but never, until nine months ago, had he been sufficiently unwell to be obliged to give up his usual avocations. More lately his attention had been attracted by a decided swelling beginning to manifest itself upon the front of his thigh. This had gone on increasing for several weeks, until it had attained its present volume. There was also a disposition, lately, for the thigh to become flexed upon the pelvis, and now the limb could not be entirely straightened. Upon examination of the patient, who was emaciated and presented a marked scrofulous appearance, I found a large fluctuating swelling, evidently sub-fascial, occupying nearly the whole anterior part of the middle third of the right thigh. The thigh was slightly flexed upon the pelvis, and the knee and foot turned a little in; the limb, generally, was emaciated. There was likewise a small swelling over the nates, behind and lower than the trochanter; this, like the larger swelling, did not in any way disappear on pressure. The swelling upon the thigh was what had given the patient most anxiety, and induced him to come to the hospital. He only wanted something done for this, as he felt well enough, as yet, to go about, and for aught else would not have been disposed to lie up. The diagnosis was at once made out as morbus coxarius. The swellings were considered to be chronic abscesses, but at first I could not determine if they communicated with the joint. The disease of the joint did not seem to have progressed very far, as the patient walked readily, and the joint functioned very well; no grating could be discovered on manipulation, nor did extensive

movement give much pain. I delayed opening the abscesses for several days, in order to watch a little the constitutional power of my patient, and further, too, to have my diagnosis more certain. I prescribed for him cod-liver oil \mathfrak{z} ss, with wine \mathfrak{z} ij, three times a day, and an anodyne at night.

April 10th.—The abscess of the thigh having materially increased in size, and the patient complaining of the feeling of tension, I made a kind of valvular opening at the dependent part, on the outer side of the sartorius muscle, and evacuated a large quantity of thin pus.

22d.—I punctured the abscess over the nates, and, subsequently to this, both abscesses having refilled, were again opened on several occasions.

May 4th.—Distinct fluctuation was discoverable higher up, behind the trochanter. A puncture was made here, and considerable pus flowed away, mixed, it appeared, with synovial fluid. Consequent upon this there supervened considerable irritative fever, with gastric disturbance and great restlessness. The oil was now discontinued, and P. gum opii, gr. j, with a glass of wine, given every three hours. The distressing symptoms were modified after a couple of days, and the opium was then only given at bed-time. The discharge from the last abscess was now very copious and offensive.

I had expressed a gloomy prognosis to some friends of the patient, and on this account I was applied to, on May 10th, to give him a discharge from the hospital, his friends preferring that he should die at home. At their earnest solicitation, a few days after, I consented to visit him privately. At the expiration of a few days I was much pleased to notice that a decided improvement had taken place under a free allowance of good wine, with nourishing diet, and opiates given when required. The discharge, though, continued profuse, and there were many evidences of extensive disease of the joint. Once or twice, when at the hospital, he heard me say something about dead bone and the persistence of the discharge. He now wanted to know if I could not in any way take out the dead bone, saying that he would submit to any operation that promised success. Eight or ten days elapsed, and finding that the discharge was as profuse as ever, moreover that ugly bed-sores were about appearing, I began to think that excision of the joint might offer a little chance. He seemed strong enough to stand an operation quickly performed, and this appeared the only alternative left. I then fairly stated to him my opinion, and told him how slight a chance the operation afforded; that it might hasten his end. He decided that the attempt should be made. I assumed the responsibility; but I determined that first I would explore the condition of the joint by making a free opening upon it through the cavity of a large abscess, just above the trochanter, and over the head of the femur. To open this abscess freely I conceived to be proper practice, even though I did not intend to resect the joint. It would be but carrying out the improved plan of Mr. Gay for treating suppurating joints. If the capsule of the joint was found open, and the head of the bone caried, then I would proceed to resect. Previous to commencing my incision, I had decided, by the test of Nélaton, that the head of the femur *was still in the acetabulum*.

On the 31st of May, 1856, my patient having been brought under the influence of ether (I used ether then instead of chloroform for the first and last time, at the suggestion of a friend; the patient took fully a pound before he became affected), was turned slightly upon the left side, and held in that position by assistants. I then thrust the point of a small amputating knife a little in front of the base of the trochanter, and carrying it upward and outward, and then downward and backward, formed a semilunar incision encircling the trochanter, and consequently had a sort of semilunar flap with the convexity upward. The knife went easily through the walls of the

abscess alluded to above, and exposed its entire cavity. The finger passed in, and to the bottom of the wound, discovered plainly considerable destruction of the capsule of the joint, and also the head of the bone, still in the acetabulum, but quite rough, and partially destroyed by caries; the brim of the acetabulum, too, could be felt considerably diseased. Under these circumstances, I conceived it best to proceed and resect the head of the femur. The point of the knife was accordingly passed across the portions of the capsule yet entire. My assistant then flexed and adducted the thigh, and, the round ligament not holding, the head was thrown out of the cavity, and then forced as much as possible through the external wound. I next passed the chain-saw behind the head and neck, and quickly divided the bone above the trochanters. Proceeding to examine the acetabulum, I was shocked at the extent of the disease. The brim was rough and crumbling, and there was an extensive perforation of the floor. With the gouge forceps I took away some portion of the brim, but soon desisted, as I felt it impossible to take away anything like the whole of the diseased structures. The wound was brought together by a few sutures, the lower end only kept open by a little lint to facilitate the exit of the discharge, the patient removed to bed, and the limb extended and kept steady by pillows. Not reacting as well as I desired, I ordered brandy freely until my afternoon visit, also a full dose of opium. In the afternoon his condition seemed better; he had warmth of skin, and a fair pulse. The brandy was continued, according to the indication, during the night, but upon my morning visit I was sorry to discover a greater disposition to collapse.

From this time he refused to respond to the most active internal and external stimulation, but sank and died, not quite thirty hours after the operation. I was not able to examine the body after death.

Appended to this case is a table, in which we have a summary account of all cases of the same kind which have been recorded up to the present time. This account shows very clearly that this operation is much less fatal than that of amputation at the joint, and that a very serviceable limb may at times be saved by it.

TABLE OF CASES IN WHICH THE HIP-JOINT HAS BEEN RESECTED FOR DISEASE.

No.	Operator.	Sex and Age of Patient.	Duration of Disease.	Condition of Patient, and State of Parts at time of Operation.	Date of Operation, and Extent of Parts removed.	Progress and Result of Case.
1	Schmalz.	Caries; head disconnected from shaft.	1816.....	Recovered.
2	White.	M. 14.	three years.	Great exhaustion; head on dorsum of ilium; fistulæ.	1818. Four inches of bone removed.	Fever slight; discharge little; health rapidly improved; well in a year; good use of limb.
3	Hewson.	Caries	1823. Resected just above small trochanter.	Perforation of acetabulum. Died in three months.
4	Schlitching.	Caries, with abscess.....	1829.....	Well in six weeks; patient able to walk.
5	Kluge.	Caries; head separated from shaft.	Died two months after operation.
6	Vogel.	Child.	Caries	Recovered.
7	Textor.	Disease of neck of bone and trochanter.	Sloughs formed on sacrum; death on fifty-third day.
8	Textor.	Caries; head out of acetabulum.	Gangrene of wound, and death on fourth day.
9	Heim.	1829.....	Recovered.
10	Brodie.	Caries; head in acetabulum.	1836.....	Died in a few days.
11	Textor.	1845. Removed all above lesser trochanter.	Complete recovery.
12	Fergusson.	M. 14.	some months.	Head on dorsum; large sinus.	1845. Four inches taken away.	Shock slight; wound healed well; health rapidly improved; limb quite useful.
13	Fergusson.	M. 8.	some time.	Emaciation and hectic; head dislocated; abscess over ilium; sinuses.	1847. Head and neck with edges of acetabulum removed.	Health improved; wound never entirely healed; died, eight years after, of liver disease.

No.	Operator.	Sex and Age of Patient.	Duration of Disease.	Condition of Patient, and State of Parts at time of Operation.	Date of Operation, and Extent of Parts removed.	Progress and Result of Case.
14	Roux.	M. 15.	long standing.	Emaciation and hectic; dislocation, no abscess or fistulæ.	1847. Head and part of neck removed.	Secondary hæmorrhage; abscess between the glutei; death on seventh day; disease of acetabulum.
15	Simon.	Child.	two years.	Head dislocated upon acetabulum; abscesses and sinuses.	1848. Head and portions of acetabulum removed.	Died four days after operation.
16	French.	F. 10.	some time.	Head carious, and upon dorsum of ilium.	1848. Head and trochanter taken away; acetabulum healthy.	Recovered.
17	Fergusson.	Recovered perfectly.
18	Walton.	M. 16.	two years.	Caries; severe pain; sinuses with discharge; some disease of acetabulum.	1848. Removed four inches of femur, and portions of acetabulum.	Pain ceased; health rapidly improved; wound healed well.
19	Walton.	1848.	Unsuccessful.
20	Smith.	M. 33.	year and half.	Head dislocated; caries; sinuses.	1848. Removed three inches and rim of acetabulum.	Symptoms improved for four months, but in two weeks more he died; Bright's disease of kidney, and caried vertebræ found.
21	Fergusson.	F. 13.	three years.	Caries; head dislocated.	1849. Removed head and trochanter major.	Recovered perfectly.
22	Morris.	M. 18.	six years.	Neck nearly destroyed; head not dislocated.	1849. Head and neck removed.	Recovered, with motion of thigh, and could walk a short distance.
23	Cotton.	F. 12.	Ulcer upon hip; caries; sinuses; emaciation.	1849. Four and a half inches removed.	Slight fever; symptoms soon improved; ulcers healed; health gaining.
24	Buchanan.	M. 41.	two years.	Great suffering; grating in joint; profuse discharge.	1850. Resected below trochanter; head and edge of cavity carious.	Recovering rapidly, but died of dysentery three months after; parts found healthy and in advanced stage of repair.

25 Skey.	F. 13.	three years.	Emaciated and nearly worn out; femur dislocated on dorsum; large ulceration on trochanter; sinuses discharging freely.	1850. Head found absorbed; portion of neck and the trochanter removed.	Result not mentioned.
26 Sayre.	Caries of head and neck.	1851. Head removed.	Wound healed, but abscesses formed; did not progress favorably.
27 Jones.	F. 32.	eighteen years.	Powers of life feeble; head carious, and on dorsum; sinuses.	1851. Head removed.	Recovered; walks some, with a high heel and a stick.
28 Stanley.	M. 18.	several years.	Feeble and emaciated; head dislocated; fistulæ discharging freely.	1852. Large abscess found under glutei.	Symptoms improved rapidly; wound not healed entirely, but health robust eighteen months after operation.
29 Hawkins.	F. 10.	four years.	Head carious; emaciation and hectic.	1852. Removed one inch below the trochanter, also edges of acetabulum.	Died on third day; found large perforation of acetabulum.
30 Bigelow.	M. 10.	Caries and dislocation.	1852. Bone separated while sawing.	Death on the twelfth day.
31 Erichsen.	M. 14.	several years.	Greatly reduced; bone dislocated.	1853. Neck and great trochanter removed.	Rapid improvement; suppuration profuse; died some months after.
32 Parkman.	M. 12.	six months.	Hectic; abscess; caries.	1853. Head and part of neck removed.	Three months after, progress very satisfactory.
33 Fergusson.	F. 13.	one year.	Head dislocated; abscess.	1854. Removed head and large trochanter.	Improvement marked.
34 Erichsen.	M. 8.	long standing.	Emaciation; hectic; caries.	1854. Removed head and edges of acetabulum.	Recovered.
35 Sayre.	F. 9.	18 months.	Caries; dislocation on dorsum; abscesses; great prostration; sweats, &c.	1854. Removed head and neck; gouged away portion of brim of acetabulum.	Recovered, with limb only quarter of an inch short; flexion, extension, and rotation preserved; general health perfect.
36 Shaw.	M. 17.	7 or 8 months.	Dislocation and protrusion of head through fistulous opening.	1856. Head removed.	Recovery, with a very useful limb; can walk three miles at a time.

No.	Operator.	Sex and Age of Patient.	Duration of Disease.	Condition of Patient, and State of Parts at time of Operation.	Date of Operation, and Extent of Parts removed.	Progress and Result of Case.
37	Shaw.	F. 14.	two years.	Dislocation; fistulæ; sinuses.	1856. Head and neck removed.	Excellent condition thirteen weeks after the operation.
38	Hancock.	M. 14.	five years.	Caries, with abscess discharging and connecting with interior of the pelvis. Probe passes from a fistulous opening in the groin into the pelvis, and then out through the acetabulum; great emaciation; night sweats and cough, with expectoration streaked with blood.	1856. Resected below great trochanter; also removed the whole floor of the acetabulum.	Patient hopping about on crutches sixteen weeks after the operation, with robust health.
39	Erichsen.	M.	1857. Head removed.	One month after the operation, the patient is doing well.
40	DeMorgan.	M. 17.	Caries of head; dislocation.	1857. Head and neck removed.	Little constitutional disturbance; patient doing well.

NOTE.—After the materials for the above table had been gathered, I for the first time saw the article of Dr. Sayre, of New York, in the 14th vol. of the 'New York Journal of Medicine.' I was pleased with his table, and so made my own conform to his arrangement in part. One or two cases recorded by him I have not been able to find reports of anywhere else, so these must rest upon his authority. I have no doubt of their correctness, and it is likely that I have overlooked them in my examination of the various periodicals in my possession. I present in my list several cases omitted by Dr. Sayre; and writing two years after him, have, of course, recorded many which have recently occurred.—R.A.K.

ART. 143.—*Case of Hydatids in the Tibia.*
By Mr. COULSON, Surgeon to St. Mary's Hospital.

(*Proceedings of Med. and Chir. Soc.*, May 25, 1858.)

CASE.—The patient, a female, æt. 25, was admitted into St. Mary's Hospital on the 20th October, 1857, with a swelling of the size of an orange in the front of the right leg, just below the tuberosity of the tibia. In the centre of the swelling there was a small ulcer, and the surrounding integuments were red and swollen. The discharge, which was not considerable, was found to contain acephalocysts. It appeared from her history that eight years previously the patient had received a kick on the front of the right tibia, a little below the insertion of the ligamentum patellæ, which injury was soon followed by a swelling, which increased in a gradual and steady manner until it attained the size of a hen's egg. Slight pain attended the growth, but no great inconvenience until four years ago, when the pain becoming severe the tumour was frequently blistered, with relief; but the swelling remained in much the same condition until the beginning of August last, at which time it first gave way spontaneously, and matter containing acephalocysts was discharged. The anterior wall of the tumour, containing a large hydatid about to escape, was removed with the saw and bone forceps, and a cavity exposed, which extended upwards within half an inch of the knee-joint and two inches or more down the shaft, and from which a considerable number of hydatids were removed. The whole of the cavity was lined by a white glistening membrane. After removal of all the hydatids to be found, Mr. Coulson rubbed the lining membrane of the cavity freely with solid nitrate of silver, and filled it with cotton wool. The woman was discharged on the 5th of February, with the wound nearly healed.

ART. 144.—*An improved method of operating in Excision of the Os Calcis.*
By Dr. MORROGH.

(*Med. and Surg. Rep.*, June, 1857; and *North American Medico-Chir. Rev.*, May, 1858.)

This operation was resorted to in a case of destructive inflammation of the os calcis and neighbouring soft parts, seriously compromising the general health, in a patient aged twelve years, who, nine months previously, had had his foot slightly squeezed between two railroad cars.

"The boy was placed on his left side on a table, and rendered insensible with chloroform.

"A vertical incision was made over the posterior extremity of the os calcis, extending from the superior to the inferior surface. This was continued along the inferior surface of the bone to its articulation with the cuboid, taking care to keep outside of the external plantar artery.

"The incision was then carried upwards to a short distance above the superior surface of the bone, without wounding the peroneal tendons. This incision described a square flap on the outside of the foot, which was dissected up, and the tendo-Achillis separated close to its attachment.

"A strong narrow scalpel was then introduced under the peroneal tendons, and made to separate the calcaneo-cuboid articulation, without severing the tendons or the artery beneath.

"The knife was then introduced between the upper surface of the

bone and the astragalus, made to cut the interosseous ligament, and gradually separate the articular surfaces. The calcaneum was then rotated, so as to bring its upper surface outward, till the internal made its appearance, when the soft structures were carefully separated, principally by the handle of the scalpel, thus completing the operation.

"After this mode of proceeding, we had the satisfaction of seeing the posterior tibial and plantar arteries pulsating in perfect integrity, while the tendons of the peroneal muscles, as well as those of the flexor longus pollicis, flexor communis digitorum, and tibialis posticus, were uninjured. No ligatures were required, the flap was approximated. A piece of flat sponge applied over the wound, and a gutta-percha splint previously moulded over the instep, so as to keep the foot extended. The operation was followed by a total cessation of pain and febrile action.

"The boy improved rapidly in health. The wound healed mostly by adhesion, the rest by granulation. The cavity partly filled up by what I suppose to be fibro-cellular tissue, and in two months after the operation the boy could run swiftly.

"At the present time he wears a small pad in the shoe under his heel, and does not experience the least inconvenience from the deficiency of the bone."

ART. 145.—*Onychia treated by ointment of Perchloride of Iron.*

By Dr. ALCANTARA.

(*Gaz. Méd. de l'Algeria*; and *Amer. Med. Monthly*, May, 1858.)

CASE.—Miss C—, æt. 19, suffered for a long time from a pain in the left foot; severe enough to interfere with walking. A recent blow had aggravated it, so as to make walking and standing entirely impossible.

The great toe of the left foot is considerably swollen; its extremity presents a very large fleshy excrescence, like a kind of pad, which covers the nail for more than half its extent. The surface of this fungus is destitute of epidermis, and a purulent exudation appears between it and the nail. The pain becomes intolerable as soon as the patient puts her foot to the ground.

The proposed extirpation of the nail frightening the patient too much, Dr. Alcantara ordered an ointment of equal parts of perchloride of iron and lard, directing it to be used in the following manner: after a local bath, to pass a bit of lint spread with this ointment between the nail and the fleshy excrescence, and also to cover with the ointment the whole surface of the toe which was stripped of its epidermis; this dressing was renewed twice a day. At the end of four days the dried and mummified excrescence came off; soon after the wound became healthy, the flesh was at its proper tint, and every thing was natural. The cure was complete on the eighteenth day and has since been permanent.

PART III.

MIDWIFERY AND DISEASES OF WOMEN AND CHILDREN.

ART. 146.—*Observations on the duration of Pregnancy.* By Dr. ELSASSER.

(*Henke's Zeitschrift*, Bd. lxxiii, 1857; and *Med. Times and Gazette*, June 12, 1858.)

As a contribution to this subject, Dr. Elsässer communicates the particulars of 260 cases of normal pregnancy with mature children, entered in the journals of the Stuttgart Lying-in-Hospital, and to which certainty may be attached:

1. *Reckoning from the day of conception*, the duration was—

Exactly 280 days in	23
Less than „ „	166
More than „ „	71
	<hr/>
	160

In nearly one half (126) the duration was between 271 and 280 days, and in 62 other cases between 281 and 290 days.

Separating the cases into primiparæ and pluriparæ, we find that there were 149 *primiparæ*, and in these the duration was—

Exactly 280 days in	14
Less than „ „	96
More than „ „	39
	<hr/>
	149

In 111 *multiparæ* the duration was—

Exactly 280 days in	9
Less than „ „	70
More than „ „	32
	<hr/>
	111

So that among primiparæ there have been greater abnormalities in plus

and minus, and the mean normal period has been seldomer met with than in multiparæ. The extremes of the duration of pregnancy (232 days, and from 301 to 306 days) occurred in primiparæ.

2. *Sex of the children.*—The sex of the child seems to have exerted little or no influence upon the duration of the pregnancy. There were born 130 children of either sex; but as among the primiparæ a great preponderance of female (85) over male (64), happened to occur, the extremes of duration were met with in them.

3. *Reckoning from the commencement of the last menstruation.*—The indication of this is often wanting, or too indeterminate to be relied upon; and among these cases it could only be depended upon in 175. Of these 13 menstruated once after conception, and the reckoning in them was made from the penultimate menstruation. The duration was found to be—

Exactly 280 days in	12
Less than " "	43
More than " "	120

175

Of the last number the duration was from 281 to 290 days in 60, 291 to 300 days in 49, and 301 to 318 days in 11.

If 280 days be accepted as the normal duration, the reckoning from the date of conception more nearly approaches it (in 8·8 per cent.) than that of the last menstruation (6·8 per cent.) This so-called normal duration is, however, that which is seldomest met with: for reckoning from the period of conception 91·1 per cent., and from the commencement of the last menstruation, 93 per cent. of all the cases either fall short of or exceed this term. The same deduction may be drawn from the observations of others. Thus, Merriman found only 9 of 114 cases in which labour came on at the 280th day. Reid enumerates only 18 of 40 women in whom, conception following a single coitus, labour came on between the 274th and 284th day: and Duncan found, reckoning from the period of conception, 275 days, and from the last day of menstruation 278 days was the mean period.

4. *The interval between the commencement of the last menstruation and conception* was found to be—

				5 days in 13 cases
From	6 to 10	"		47 "
"	11 " 15	"		38 "
"	16 " 20	"		18 "
"	21 " 25	"		9 "
"	26 " 30	"		14 "
Above	. . 30	"		8 "

These statements are, however, to be taken with the greatest caution, for it is seldom that this class of women observe exactly the last day of menstruation.

5 *The weight of the child in relation to the duration of pregnancy.*—Dr. Elsässer furnishes a table in which the mean weights of the children are placed side by side with the duration of the pregnancies of their mothers; but we need not transcribe this, inasmuch as it only proves that there is no relation prevailing between the two facts.

ART. 147.—*How many children can a woman bear?* By Dr. SZUKITS.

(Zeitschr. der Gesellsch. der Aertze zu Wien, July and Aug., 1857.)

Dr. Szukits says this question has not yet been satisfactorily answered. He himself has observed two females, each of whom had borne 24 children. Osiander ('Handb. d. Entbindungs Kunst,' 1 Theil, 1 Abth., S. 319) mentions one woman who, during her married life, bore 42 children, and another who had 53. Burdach ('Die Physiol. als Erfahrungswissenschaft,' 1 Bd., 1826, S. 410) relates that the wife of a countryman in the Moscow district had given birth to 69 children at 27 confinements; four times 4 at one birth, seven times 3, sixteen times twins. In the year 1809, the Vienna newspapers contained the following announcement: Maria Anna Helm, the wife of a poor linen-weaver in Neulerchenfeld, twenty years married, bore at 11 confinements 32 children; 28 living, and 4 dead; 26 were males, and 6 females; all were begotten by one man, and nursed by herself. She had at her last confinement 3 children; 1 living and 2 dead. Her husband was a twin, she herself one of four. Her mother had produced 38 children, and died during a confinement with twins (Osiander, 516.) Six children seem to be the largest number ever produced at one birth. A perfectly trustworthy instance of this is the following: The 'Schwäb. Mercur,' No. 8, S. 22, 1806, contains the following notice: Ohlau in Silesia, 11 Dec., 1805. The wife of a chimney-sweep here, named "Döpfer," was yesterday confined of six children; all were boys, and dead. This woman, who has been twice married, has already given birth to 44 children. During her first marriage, which lasted twenty-two years, she bore 27 boys and 3 girls. In her second marriage, which has lasted but three years, she has borne 14 children—3 at the first, 5 at the second, and now 6 at third confinement (Osiander, 320.)

ART. 148.—*Statistics of operative Midwifery in private practice.*
By Dr. MEISSNER.

(Monatsch. f. Geburts-Künde, Band ix, 1858; and Medical Times and Gazette, May 29, 1858.)

This is a report by the great Leipzig veteran, on the results of his thirty-five years' midwifery practice, as far as operative midwifery is concerned. He observes, that a review of private midwifery practice must be taken from quite a different point of view to that of a public institution; for while, in the latter, the bulk of the cases are examples of natural labour, the private practitioner is only consulted, as a general rule, when the case has become pathological. [These observations are, of course, only applicable to the mischievous Continental practice of leaving cases in private practice to the almost exclusive care of widwives, the physician only being called in when the case has assumed a serious aspect, and, of course, frequently too late.] Moreover, in the Lying-in Hospital, the physician treats his cases in a suitable locality, with all he wants at hand; while, in private, the practitioner is called miles away to neglected cases, having insufficient appliances, and is forced to treat his cases under the most disadvantageous circumstances.

Dr. Meissner's observations relate to 3811 women, who, he says, gave birth to 3980 children, as 136 were twin, and 2 triplet births. But there is here obviously some error in the numbers, as these do not correspond with each other. Of the 136 twin-births, in 54 there were 2 boys, in 35, 2 girls, and in 47 children of both sexes. Of the triplets, in one case there were 2 boys and 1 girl, and in the other 3 girls. These 3811 labours called for operative interference in 3025 instances, dynamic aid being required in 924 of these to effect change of position. There were 1863 forceps operations (!), this instrument being required in five other instances for the removal of separated fungoid or polypous tumours. Turning was performed 351 times, extraction 247 times, perforation 32 times, the induction of premature labour 20 times, the Cæsarean operation 6 times, *accouchement forcé* 55 times, polypus operations 6 times, placenta separation or deliveries 447 times. 2086 boys and 1684 girls were born at full time, while there were 265 premature births or abortions, and 24 moles and polypi. 399 children were born dead, and of those living 36 died within fourteen days. Twenty-five mothers died either before or during labour, or within sixteen days after it.

Turning.—Although this, when performed at the proper time, is usually an easy operation, executed within a few minutes, yet of the author's 351 cases in only 222 did it prove completely successful for both mother and child. In 104 cases the child was born dead, in 2 cases the mother died from the delivery, and in 2 after this had taken place. This issue is chiefly to be attributed to the ignorance of midwives in the detection of cross presentations, and the delay that elapsed before aid was obtained. Dr. Meissner prefers turning by one foot, unless there is some indication for hastening the delivery. This rule is of especial importance when multiple births are expected, so as to avoid getting hold of two feet of separate children. In one of the author's cases of triplets all the children presented cross-wise, and six lower extremities were felt. All the children were safely delivered by operating upon one foot at a time. The one-foot delivery also gives the child the best chance of being born alive. *Cephalic* version is preferred by some practitioners, as giving the child the best chance; but it can only be tried when hastening the delivery is not an object. The author resorted to it successfully in 6 cases. In 4 of the 351 cases turning was performed by *external manipulation*, and in 3 by *spontaneous* version, the buttocks being forced by the presenting shoulder into the pelvis, and all the children being born dead.

Forceps operations.—Of 1863 cases, in 1750 the head was the presenting part; and in 113 the forceps were used for its delivery in other presentations. Of the 1754 children delivered, 90 were born dead, *i. e.*, nearly 12·19. This, in an operation which, carefully performed, can hardly be considered dangerous to life, is a high proportion; but the child had, in fact, not unfrequently died during pregnancy, and was in advanced putrefaction on delivery. In 10 instances the child was hydrocephalic or dropsical; in 3 the mother was already dead; there were 5 cases of spina bifida; 7 deaths took place from prolapsed funis, and 3 from the arm and head being tightly wedged in together. In 4 instances the mother had suffered from repeated convulsive paroxysms. In other cases the application of the forceps had been repeatedly attempted by

preceding practitioners, or the passage was narrowed by the presence of tumours. Dr. Meissner remarks, by the way, that the separation of the epidermis is not always a certain sign of the death and putrefaction of the child, as it may be produced by an acrid condition of the *liq. amnii*.

Extraction.—This was performed in 247 cases, and became necessary when, in breech, knee, or foot presentation, the child's life was threatened by cessation of pain, faulty position, or prolapsus of funis. Only 145 of the children were born living; but then, 13 had died through pressure on the funis, before the author's arrival; 18 were in a state of putrefaction, 10 were already born except the head, 8 were immature, and in 5 others there was hydrocephalus or other form of dropsy. In 42 instances extraction had to be performed on account of cessation of pain after turning.

Perforation.—In his thirty-six years' practice, this operation has only been performed by the author thirty-two times, and he had attended 3299 labours before he had his first case. He has always followed the maxim laid down by the chief German practitioners, of never proceeding to the operation until assured of the child's death; and it has several times happened to him to see living children born in cases which have been left for days together to the powers of nature, and which in previous labours had been delivered by perforation.

Premature labour.—So much has the performance of the operation for premature labour limited that of perforation, that while to the year '42 he had had to resort to perforation in twenty-eight instances, he has during the subsequent fourteen years only performed it four times. He has never induced premature labour prior to the thirty-sixth week, and has never found this too late, the bones continuing thus long sufficiently soft and yielding to accommodate themselves to the narrowed pelvis. When the reckoning is uncertain, he performs the operation thirty-six weeks after the last menstrual period.

Forced delivery (accouchement forcé).—By this term, the author understands the whole series of operations (as artificial dilatation of the os uteri, bursting the membranes, turning, extraction, or removal of placenta) which may be required for delivery when the further continuance of pregnancy is dangerous to mother and child. It is especially called for in certain cases of eclampsia, placenta prævia, and obstinate vomiting. He enumerates under this head two instances of opening the adherent os uteri by means of the knife, and eight cases of its forcible dilatation. This last procedure was resorted to because, after the labour had continued three or four days, in place of dilatation of the os uteri, general debility and delirium set in. The author has resorted to it fifty-five times during thirty-six years. In thirty-three of these cases both mother and children did well, although, as the dilatation was usually undertaken for placenta prævia, most of the latter were born some weeks too soon. As the majority of cases (31) were examples of placenta prævia, in which hæmorrhage had continued long before the patients were seen by the author, it is not surprising that ten of the mothers died; but another statement of the author, that even when he arrived at his patients he plugged the vagina, and awaited pains before proceeding to deliver, is somewhat extraordinary.

Cæsarean operation.—Of the six examples of this that have fallen to the author's lot, five were performed on mothers being already dead, the children being saved in none. In the case of operation upon the living subject, both mother and child lived.

Placenta removals.—The number of these (447) must seem very large; but it is to be remarked that, perhaps not one tenth of these cases were examples of abnormal adhesion requiring separation, the placenta being frequently detained from other causes, preventing the success of the ordinary manipulations, as "stricture of the uterus," spasmodic contraction of the uterus from too early interference, &c.

General results.—Of the mothers, 41 were lost, 25 during, and 16 after delivery. Of the former 25, 11 were already lifeless when seen; and of the 14 others, 1 died from rupture of the omentum with internal hæmorrhage, 10 from placenta prævia necessitating forced delivery, 2 from nervous shock after favorable labour, and 1 from hæmorrhage. Of the 16 mothers who died after delivery, 1 died from cancer of the stomach, 1 from pneumonia, 3 after repeated attacks of eclampsia, 1 from putrescence of the uterus, 1 from typhus following the birth of a putrid premature child, 4 from puerperal fever following operative procedures, 1 from "paralysis of the lungs," 3 from the consequences of loss of blood, and 1 after several hours' operative attempts by a country practitioner.

Of the *children* 399 were born dead, as already stated, under the various operations. Besides these, 36 died within the first fourteen days after birth; viz. 4 from debility from too early birth, 6 from atelectasis pulmonum, 2 from trismus, 1 from fissure of the cranium after a forceps operation, 1 from chronic hydrocephalus, and 2 from want of breast-milk.

After remarking upon the remarkable sequences met with in practice of unusual pathological occurrences, and of the operations required for their relief, the author observes, that, as a general rule, forceps operations are found to be most frequent in cold, changeable weather, which induces rheumatic affections of the uterus, not only rendering the dilatation of the os very painful, but delaying its accomplishment for days. This condition may be often prevented by clothing warmly the lower part of the person; and when it is present it should be treated by Dover's powder. The standing too much over the fire, also, may, by over-heating the abdomen, lead to a plethoric condition of the anterior wall of the uterus, which may not be without its influence in inducing morbid adhesion of the placenta. Such an occurrence is best prevented by abstaining from this practice, and bathing the abdomen with cold water. When adhesion has taken place repeatedly at the same place, in consequence of an indurated condition of a portion of the uterine wall, we should, after the termination of the puerperal condition, endeavour to induce absorption by mild mercurial or iodine frictions, tepid baths, together with hemlock and mallow injections. If these do not succeed, the baths at *Krankenheil*, near *Tolz*, in *Bavaria*, which have been found so useful in fibroid and hypertrophy of the uterus, should be tried.

ART. 149.—*On the prevention of laceration of the Perinæum.*

By Dr. MATTEI.

(Vierteljahrssch. für Prak. Heilk., 1858, and Med.-Chir. Review, Oct., 1858.)

Dr. Mattei gives the following views on the means of preventing laceration of the perinæum. It is especially necessary that the head pass the vulva in a favorable direction. This can only happen when it passes with the necessary degree of flexion. Whilst the occiput passes under the pubic arch, the face has not yet quitted the pelvic outlet; first when the upper part of the neck comes under the pubic arch, can the extension of the head (or the separation of the chin from the breast) begin. If the distension of the perinæum begins too early, the head must pass the vulva with unfavorable diameters; namely, with the great oblique, or great or straight diagonal diameters. Such a passage easily causes laceration. Hence it is the task of the physician to prevent a premature distension by the head. This he effects by placing two fingers between the labia, or, in some cases, between the pubic arch and occiput, so as to bring the head downwards and outwards, at the same time laying the other hand on the hinder part of the perinæum, upon which the face is lying, and pushes this upwards. This manœuvre is to be executed during the pains, which will thus protrude the head forwards in the requisite arc. A very simple means of expediting the birth of the head consists in compressing firmly the distended perinæum with the whole hand. This resembles the squeezing out of the kernel from a cherry. On the passage of the shoulders care must also be taken lest the two shoulders pass together.

ART. 150.—*History of a Forceps Case.*

By Dr. ROBERT LEE, Obstetric Physician to St. George's Hospital.

(Medical Times and Gazette, Sept. 18, 1858.)

CASE.—At 10½ p.m. on Monday, August 23d, 1858, I was requested to see a lady, æt. 40, who was stated to have been in labour since the Saturday morning. "The head on the perinæum; the pains have gone off; she looks well, but the pulse is 120, and there has been no progress during twelve hours. When the membranes gave way not precisely known." At 11 p.m. pulse rapid; no pain whatever. The head pressing upon the perinæum—the external parts partially dilated. An ear under the symphysis pubis readily felt. There was a peculiar fœtor in the discharge from the vagina. Auscultation was employed, but the sound of the fœtal heart could not be heard. Had I been absolutely certain that the child was dead, I would not have applied the forceps, although it was a favorable case for delivery with the forceps. The movements of the child had been but little felt by the mother during the day. I applied the blades of the forceps readily, and got the head nearly in the world, when the perinæum appearing to be in great danger, I took off the blades, and by slight pressure with the fingers on the sides of the head in place of the blades, I easily extracted it. A bloody fluid escaped from the mouth and nose. The skin of the abdomen was peeling off. It must have been dead at least two days. The labour had commenced at 4 a.m. on

the Saturday morning. It went on all the Saturday, Sunday, and Monday, till Monday night at 11 p.m. It was the first child.

Two ounces of chloroform had been given during the Saturday and Sunday. Once the patient was nearly insensible. On inquiring why this had been done, the medical attendant stated that he did not approve of it, but the patient insisted upon having it. She informed me that a lady of her acquaintance was attended by "a chloroform doctor," and that she had, in consequence of this, contrary to the advice of her medical attendant, insisted upon taking it. It appeared almost certain that if he had not yielded to the wishes of his patient, she would have placed herself in other hands.

I observed to Dr.—, after the delivery with the forceps of a child that had been dead two days, that it would be most important in cases of protracted labour, if any means could be discovered by which the life or death of the child could be determined with absolute certainty. The method of treatment in many cases would be regulated by this. Had I been certain in this case that the child was dead, I would not have delivered with the forceps, but by craniotomy.

Since the occurrence of this case I have applied the stethoscope over the anterior fontanelle immediately after birth, but the pulsation of the arteries of the brain was not heard. I tried the instrument recently invented by Dr. Alison, but this was equally unsuccessful.

The 12th chapter of M. Mauriceau's second book is entitled, "*Les signes qui font connoître que l'enfant est vivant ou mort dans la matrice.*" He was fully aware of the importance of the subject, and among other expedients recommended by him to ascertain the fact with certainty, was introducing the hand into the uterus to feel whether there was any pulsation in the arteries of the umbilical cord, or in the artery at the wrist. "*Et si mettant la main dans la matrice, on trouve l'enfant froid,*" says M. Mauriceau.

"A sense of coldness in the abdomen," is one of the symptoms enumerated by Dr. Merriman as among those which are useful "in proving that the fœtus has been dead in utero for several days or even weeks." Among the "signs of a dead child," the eleventh mentioned by Smellie is, "a coldness felt in the abdomen."

I felt curious to know whether the temperature of a dead child during labour was different from that of a living child, for during labour the fact could readily be ascertained. I soon found that the temperature of a living child immediately after birth was 98°. In a case of twins, the feet presented. I measured the temperature of the feet and thighs before the nates were expelled, and the heat was 98°. After the expulsion of the breech, the thermometer was introduced into the anus, and the heat was 98°. After the birth of the child I found the temperature of the axilla, mouth, and head the same. I ruptured the second bag of membranes, and when the head was expelled, but not the body, I put the thermometer in the mouth, and it was 100°. The heat of the mother's mouth and vagina was 98°.

I have not had an opportunity of ascertaining what the temperature of a dead child is, either before, during, or after delivery.

ART. 151.—*History of a case of Craniotomy.*

By Dr. ROBERT LEE, Obstetric Physician to St. George's Hospital.

(*Medical Times and Gazette*, Oct. 20, 1858.)

CASE.—On Sunday morning, at half-past one, of the 26th September, 1858, I was called to an obstetric case. The patient was 31 years of age; first

child. The practitioner had first seen her at half-past six o'clock on the Saturday morning. Labour, it was stated, had been progressing slowly ever since. "All to-day fæcal matter has been passing per vaginam. She appears to be getting exhausted. Pulse 120; tongue furred. It appears to be a case where there is nothing left for us but perforation."

The husband, who came with a letter from the medical attendant, was in a state of great alarm, and said in driving home, "Will it be necessary for you to give my wife chloroform?" My answer was, "Certainly not; I have never seen chloroform do the slightest good in any case of midwifery, and in some the greatest mischief." On reaching the house, I found two medical practitioners in attendance. The circumstance which had excited the greatest anxiety was, "the fæcal matter passing per vaginam." They assured me it was not the meconium, but the contents of the mother's bowels which were escaping. I inquired if there was any symptom of rupture of the vagina, but there had been no vomiting, and the head was in the pelvis, and actually resting upon the perinæum. One of the medical attendants spoke of the forceps, but did not actually propose to use the instrument. I inquired if the forceps had ever been applied by him to the head of a child positively known to be dead, which was the fact here. To this question no reply was given; but it was my impression he had never done this.

I found the patient a good deal exhausted, and the pulse rapid. The pain had entirely ceased. The discharge from the vagina extremely fœtid. The head was so flaccid, and the bones so much pressed over one another, that there could be no doubt the child had been dead for a considerable period. I examined the posterior wall of the vagina; but there was no rent, and no feculent matter was passing per vaginam. I had not a thermometer with me to determine exactly the temperature of the head; but it did not feel colder than the vagina. No chloroform was given before proceeding to perforate and extract the head, which was speedily done. There was no thermometer in the house; but I applied my hand to the body of the child immediately after its escape, and there was no sensible difference in the temperature between this and a living child. Both practitioners did the same, and we all came to the conclusion that the temperature must have been about the same as that of the mother. The bandage was applied, but the placenta did not come away for about half an hour; and as it adhered was removed by the medical attendant very carefully and successfully; slight hæmorrhage followed, but soon ceased.

The forceps would have been employed in this case, had the child been certainly known to be alive, or if its death had remained doubtful.

It might now, I think, be considered as an aphorism in midwifery—that the forceps is not applicable to dead children, nor in cases where the os uteri is not fully dilated, and the head has not descended into the cavity of the pelvis and can be felt. But I had forgotten, the aphorisms in midwifery have all of late been turned topsy turvy.

ART. 152.—*On the comparative use of Ergot and the Forceps in Labour.*
By Dr. FORDYCE BARKER.

(*Amer. Med. Monthly*, July, 1858.)

The more enlarged our clinical experience, and the more accurate our observation, the more rarely, in Dr. Barker's opinion, shall we have recourse to ergot before delivery. Delay in labour, moreover, is held to be more dangerous than promptness in the use of the forceps. Ergot

is said to be safe only in those cases where the presentation is natural, the pelvis well formed, the os uteri well dilated, the vagina and vulva lax and moist, and, in short, everything prepared for delivery, nothing being wanting but efficient action of the uterus. The conclusion is, that the dangers are many and great, and the advantages few. On the other hand, the dangers resulting from the use of the forceps are set down as overrated, and the dangers of *delay* as greatly underrated. The plain, practical question for the lying-in-room, Dr. Barker thinks, is—which is safest for mother and child, *the use of instruments, or further delay?* And to this question the answer is to be found in the accompanying quotation:

“Professor Simpson has shown that the maternal mortality attendant upon parturition increases in ratio progressive with the increased duration of the labour. He has made out the following table, showing the proportion of 138 natural deaths in relation to the duration of labour in 15,850 cases of delivery recorded by Dr. Collins:

Duration of labour.	No. of deliveries.	No. of deaths.	Proportion of deaths.
“ Within 1 hour	3537	11	1 in 322
From 2 to 3 hours	6000	26	1 in 231
From 4 to 6 hours	3875	29	1 in 134
From 7 to 12 hours	1672	21	1 in 80
From 13 to 24 hours	502	19	1 in 26
From 25 to 36 hours	134	8	1 in 17
Above 36 hours	130	24	1 in 6

“So also the infantile mortality attendant upon parturition increases in ratio progressive with the increased duration of the labour, as is shown in the following table of the proportion of stillbirths, in reference to the duration of labour in 15,850 cases of delivery:

Duration of labour.	No. of deliveries.	No. of stillborn.	Proportion.
“ Within 2 hours	7050	347	1 in 23
From 3 to 6 hours	6362	346	1 in 18
From 7 to 12 hours	1672	151	1 in 11
From 13 to 24 hours	502	88	1 in 6
From 25 to 36 hours	134	42	1 in 3
Above 36 hours	130	71	1 in 2

“It will thus be seen that the dangers of delay, both to mother and child, become a question of the gravest importance. Among our systematic authors, Burns has more strongly, and I think more truly, pointed out these dangers than any other of our English writers. He says the continued pressure of the head on the soft parts is productive of further diminution of the capacity of the pelvis, for inflammation is excited, and at the same time the return of the blood by the veins is obstructed, and of serum by the lymphatics. This impairs the power of the soft parts, and renders the inflammation of the low kind, so that even when delivery is accomplished sloughing succeeds, whereby very dreadful or loathsome effects are produced, if these, indeed, be not prevented by the death of the patient, in consequence of a similar low

inflammation being communicated to the peritoneum. This swelling of the parts contained within the pelvis may take place, although the head be not impacted, but the head cannot long be impacted without producing that.

“Here, then, is one effect of a most formidable and alarming nature, which we apprehend in the case under consideration. But this is not the whole of the evil; for the upper part of the vagina, or the cervix uteri, may be lacerated in consequence of this debilitated state, or any part of the uterus may be ruptured by strong and spasmodic action; or uterine or peritoneal inflammation may be excited previous to delivery, proving fatal in a few hours after labour is terminated; or hæmorrhage may occur, to a fatal degree, from want of energy in the uterus after delivery; or general inanition and exhaustion are produced; the pulse becomes frequent, and at last feeble; the mouth parched; the skin hot; the mind confused, and the strength sunk; or the powers of life may be worn out, so that the patient shall die without any decided inflammation or disease referable to a common nosological system. In the ‘Clinical Midwifery’ of Dr. Robert Lee, who is no advocate for the frequent use of the forceps, and, indeed, who never uses them except when the head is at the lowest strait, occurs the following statement, which seems to me very significant: ‘In thirty-eight cases of this report the labour continued from forty to seventy hours. In the cases of spontaneous rupture of the uterus and convulsions only was the delivery effected before the labour had lasted upwards of thirty hours. In a very large proportion of the cases the difficulty arose from distortion, or a contracted state of the pelvis. Rupture of the uterus took place in three before perforation; and the inflammation and sloughing of the uterus, vagina, and bladder, which proved fatal in eight hours, were chiefly or solely produced by the long-continued violent pressure on the soft parts, by the head of the child before it was opened and extracted. In those who recovered with vesico-vaginal fistula, or contraction of the vagina from cicatrices, the unfortunate occurrence arose from craniotomy being too long delayed.’ In eighty-seven of Dr. Lee’s cases, where craniotomy was performed, local lesions on the part of the mother are noted as having occurred in several instances. Out of the eighty-seven cases, eight, or about one in every ten, suffered from vaginal inflammation and sloughing; four, or nearly one in every twenty, were left with vaginal fistula. In a paper on the subject of ‘Urethro-vaginal and Vesico-vaginal Fistulas,’ published in the ‘North American Med.-Chir. Review’ for July and November, 1857, by Dr. N. Boseman, of Montgomery, Alabama, he states, that in nineteen cases of these fistulas ‘the shortest duration of labour in any one of these cases was thirty-six hours, and the longest eight days; the average being about four days. In nine of these cases instruments were employed to aid in the delivery; in six no artificial means were resorted to.’ He adds: ‘Judging from the nature of the fistulous openings in the cases where instruments had been used, and where they had not, I am forced to the conclusion that nearly if not all of them were the result of sloughing.’ In further confirmation of the views advanced as to the danger of delay in labour, I add a note from Dr. Sims, who has undoubtedly had a larger experience in the lesions resulting from parturition than any man living:

“ ‘My dear Doctor—Out of about one hundred and twenty cases of vesico-vaginal fistula, I have had time to look over the histories of only seventy. Of these, forty-one were delivered by instruments, the rest being left to the unaided efforts of nature.

“ ‘These fistulas are sometimes produced by laceration, but most commonly by a slough, which is generally in proportion to the duration and degree of impaction, whether instruments are used or not. Instruments are often blamed for injuries which are produced, not by their use, but by the want of their timely application; in other words, by the prolonged pressure resorted to.

“ ‘The cases left entirely to the unaided efforts of nature, other things being equal, suffered the greatest loss of structure; those in which instruments were used sustained, as a rule, less loss in proportion as they were resorted to early or late, thus showing that the mischief was the result of prolonged pressure.’ ”

ART. 153.—*A new method of treating prolapsus of the Funis.*
By Dr. T. GAILLARD THOMAS.

(*Transactions of the New York Academy of Medicine*, vol. ii, part 2, 1858.)

In a course of lectures on obstetrics, delivered by Dr. Thomas in the University Medical College of New York, about two years ago, he investigated this subject, and came to the following conclusions: first, that the causes of the persistence of this accident (whatever may at first have produced it) reduced themselves to two, the slippery nature of the displaced part and the inclined plane offered it by the uterus, by which to roll out of its cavity;* and secondly, that the only rational mode of treatment would be inverting this plane, and thus turning to our advantage not only it, but the lubricity of the cord, which ordinarily constitutes the main barrier to our success. This he found could be readily accomplished by placing the woman on her knees, with the head down upon the bed, in the posture assumed by eastern nations in worship, and now often resorted to in surgical operations upon the uterus and vagina. Let it be remembered that the axis of the uterus is a line running from the umbilicus, or a little above it, to the coccyx, and it will be seen (here are two diagrams which make the point very evident) that by placing the woman in this position it will be entirely inverted.

Dr. Thomas then relates three cases in which he carried out this practice easily, and saved the life of the child, and after this he proceeds to lay down certain rules, which are—

1. That if the cord be detected in the unruptured bag, the woman be at once placed in position before escape of the waters, and that no efforts at return of the prolapsed part be made by the hand. The position alone will, I believe, cause its return to the uterus; and if it does not, we may do so manually as soon as the waters escape.

2. That if the pelvis be so fully occupied by the presenting part

* When the woman is placed on the side, the axis of the uterus is not so favorable to prolapse as when on the back; still it aids very much in causing the accident.

as to preclude return of the cord by the hand, a gum elastic catheter and tape to be used as a porte-cordon.

3. That no manipulations be commenced until the woman be placed in position.

4. That in returning the cord the whole hand be introduced into the uterus; the fingers alone will fail.

ART. 154.—*Port-wine enemata as a substitute for transfusion of blood in post-partum hæmorrhage.* By Dr. LLEWELLYN WILLIAMS, of St. Leonards-on-Sea.

(*British Med. Journal*, Sept. 4, 1858.)

CASE.—On September 22d, 1856, I was called into the country a distance of four miles, to attend Mrs. C—, æt 42, then about to be confined of her tenth child. All her previous accouchements had been favorable. When about six months advanced in pregnancy, she received a violent shock by the sudden death of her youngest child, since which time her general health had become much impaired. She had a peculiar pasty anæmic appearance, and complained much of general weakness.

On my arrival I discovered the os uteri fully dilated; the membranes ruptured spontaneously; and after three or four powerful pains, a fine female child was born. Placing my hand on the fundus uteri, I felt it slowly contracting under my grasp. My patient exclaimed "I am flooding away," and fainted. I immediately had recourse to such restoratives as were at hand, and presently she began to revive. On making an examination, I found the placenta lying detached in the vagina, and removed it without difficulty, together with a large quantity of coagula. I had administered a dose of volatile tincture of ergot. The uterus continuing to contract feebly, and more than the usual amount of discharge being present, I applied some cold cloths to the vulva and hypogastric region; this having little apparent effect in arresting the discharge, though steady pressure was continuously applied with the hand on the abdomen, I had recourse to the plan recommended by Gooch, of throwing a quantity of cold water suddenly on the abdomen. My efforts still being foiled, and the hæmorrhage continuing, the powers of life manifesting evident symptoms of flagging, I introduced my left hand into the uterus, after the manner also recommended by Gooch, endeavouring to compress the bleeding vessels with the knuckles of this hand, whilst with the other I pressed upon the uterine tumour from without. This combination of external and internal pressure was equally as unavailing as any of the other plans already tried. At last, by compressing the abdominal aorta, as recommended by Baudelocque the younger ('*Mémoires de l'Académie des Sciences*, January, 1835), I was enabled effectually to restrain any further hæmorrhage. The condition of my patient had now become sufficiently alarming, she having been for upwards of half an hour quite pulseless at the wrist, the extremities cold, continual jactitation being present, the sphincters relaxed, and the whole surface bedewed with a cold clammy perspiration. It now became a question what remedy could be had recourse to, which should rescue the patient from this alarming state, it being utterly impossible to administer any stimulant by the mouth. My distance from home, together with considerable objections to the operation itself, which it is not here needful to dwell upon, made me abandon the idea of transfusion of blood; but, as a means which I believe will prove equally as powerful as

transfusion in arresting the vital spirit, I had recourse to enemata of port wine, believing that this remedy possesses a threefold advantage. The stimulating and life-sustaining effects of the wine are made manifest in the system generally; the application of cold to the rectum excites the reflex action of the nerves supplying the uterus; and the astringent property of port wine may act beneficially by causing the open extremities of the vessels themselves to contract.

I commenced by administering about four ounces of port wine, together with twenty drops of tincture of opium. It was interesting to note the rapidity with which the stimulating effects of the wine became manifest on the system. Two minutes after the administration of the first enema, there was a slight pulsation distinguishable in the radial artery, which perceptibly increased in strength for the space of five minutes, after which the pulse again began to flag, and I had recourse to the administration of a second enema twenty minutes after the first. A more marked improvement was now manifest in the patient. She regained her consciousness; the pulse continued feebly perceptible at the wrist. In half an hour I had again recourse to the enema, with the most gratifying result; and, after ten hours' most anxious watching, I had the happiness of leaving my patient out of danger. The quantity of wine consumed was rather more than an ordinary bottle.

ART. 155.—*On the treatment of Uterine Hæmorrhage.* By the late Dr. LABATT, late Master of the Rotundo Lying-in Hospital, Dublin.

(*Dublin Quarterly Journal of Medicine*, May, 1858.)

The following remarks, which have much practical value, occur in a paper edited by Mr. Hamilton Labatt, the son of the writer:

"First," says Dr. Labatt, referring to a case which he has just related, "I kept the patient out of bed, as advised by Dr. Denman, till the child was on the point of being born, being of opinion, with Dr. Denman, that the erect position is favorable to uterine contraction. Secondly, I allowed the child to be entirely expelled by the action of the uterus, and even opposed some resistance to its progress through the passages, and immediately after administered a cordial anodyne draught, which I have for many years been in the habit of doing in such cases, and often with great advantage, always combining the opiate with a cordial. Thirdly, I firmly held the uterine tumour in my grasp for several hours, and then applied a pad and roller. It was the invariable practice of all the old practitioners in this city to give a glass of burnt brandy with nutmeg, immediately after delivery, and I think such a cordial will often be found beneficial, by promoting uterine contraction, and thereby favouring the separation and expulsion of the placenta, and I cannot say that I have ever known it to produce injurious consequences."

And in another place:

"Although I do not at present mean to treat of the general management of uterine hæmorrhage, I think it well to offer a few remarks on the use of opium, which has of late been largely used by some practitioners. I have read accounts of cases of flooding, in which seven or eight hundred drops of tincture of opium were given within six or eight hours, and, as alleged, with the best effects. The opinion which I had

long held respecting the nature of the proximate cause of uterine hæmorrhage in childbed, and the effects I had often witnessed of large doses of opium on uterine action, led me to suppose that it was not likely, in such large doses, to prove beneficial in the complaint in question. However, the extraordinary success attributed to the practice by some respectable practitioners led me to give it a fair trial, and the result has been a conviction on my mind that opium ought not to be given too freely in floodings attended with great weakness. I have generally observed that, when administered in very large doses, it increases the weakness, disorders the stomach, and tends to suspend the healthy uterine contraction, and prolong rather than check the discharge. It is good practice to give a moderate dose of black drop, or Batley's sedative liquor combined with burned brandy, volatile aromatic spirit, or Hoffman's liquor, immediately after the birth of the child, when we have reason to apprehend flooding, and the same may be repeated if necessary.

"It is generally supposed that if, after the expulsion of the placenta, the uterus be felt well contracted, small, round, and firm over the pubis, there is no reason to apprehend hæmorrhage; this may be true to a certain extent; nevertheless, if due care be not taken to maintain this state of salutary contraction by the means already advised, the uterus may relax, and hemorrhage ensue; therefore, the cautious attendant will patiently continue his preventive measures till all immediate danger of hæmorrhage shall have subsided, and then apply his pad and roller.

"I would here beg to offer a suggestion, for the guidance of the young practitioner in his attendance during the anxious and critical period of childbirth. I would strongly urge him to remain with his patient as much as he possibly can, from the commencement of labour to its termination; his presence will be acceptable to the friends of the patient, comfortable and cheering to herself, and she will have the benefit of seasonable advice and assistance on any of those emergencies which every now and then unexpectedly occur in the progress of labour. But this is not all: the presence of the confidential medical attendant, and his humane and kind deportment, will inspire the sufferer with confidence and hope, and thus, by the well-known influence of mental impressions on the action of the uterus, will tend to lead the case to a speedy and happy issue. I do not pretend to say that such is the invariable result, for I have known cases to go on slowly where the practitioner remained for hours on the spot, and where, during his short absence, pains suddenly increased, and accomplished the delivery before his return; but I have seen more than sufficient to justify me in asserting, that if accoucheurs would remain more at the bedside of the patient than is the practice of the present day, and sometimes pretend to assist, with the intention, as Dr. Denman remarks, of giving confidence to the patient, or composing her mind, the duration of labour and sufferings of the patient would often be abridged; and I dare to say that the success of Dr. Hamilton, who assures us that 'no patient under his charge for the last thirty-five years has been above twenty-four hours in labour, and excepting in cases of disproportion, none so long,' was more owing to this circumstance than to any direct manual aid he may have afforded."

ART. 156.—*On Puerperal Convulsions.* Dr. R. U. WEST, of Alford.

(Assoc. Med. Journ., May 26, 1854.)

In this paper Dr. R. U. West holds that all cases of genuine convulsions depend more or less on *irritation* of some kind; that cerebral congestion is to a greater or less extent induced in all; that that is the condition which is so uniformly relieved by bleeding; and that, for the sake of applying “the principle of removing the cause of the convulsions, of substituting new modes of irritation different from that which has produced the convulsions,” to repeat the language of Denman, it is suggested that the most useful practical classification of convulsions would be:

CLASS I. *Irritation* uterine. Convulsions *during* labour, and generally ceasing on the termination of the process. *Treatment*: bloodletting to remove induced congestion; but chiefly, speedy delivery.

CLASS II. *Irritation* extra-uterine, and interfering *indirectly* with the establishment of the labour through the diverted nervous influence. Convulsions *preceding* labour, which appears to be imminent, and ceasing or becoming materially milder on the accession of true labour. *Treatment*: bloodletting as in class I, and for the same reason; but chiefly the induction of the labour process, which will probably prove to be the *natural counter-irritant*, while at the same time attention must be paid to the removal or alleviation of ascertained or probable causes of extra-uterine irritation. Other counter-irritants, such as sinapisms, may also be used.

A MIXED CLASS, between these two. *Irritation* probably uterine, and consisting of some cause, such as excessive rigidity of the os uteri, calculated to prevent the parturient process from going on normally. Convulsions *before and during* labour. *Treatment*: bloodletting as before, and for the same reasons; but chiefly the removal of the cause which appears *directly* to hinder the process from going on.

CLASS III. *Irritation* sometimes mental, sometimes of a physical extra-uterine nature. Convulsions coming on *after* the labour, the parturient process having possibly, by its *natural counter-irritant* effect, prevented the play of the pernicious influences now set at liberty. *Treatment*: bloodletting as before, and for the same reasons; and, as far as possible, the removal or alleviation of probable sources of irritation; while, as in class II, different forms of *artificial* counter-irritation may also be tried.

A MIXED CLASS, between the last two. *Irritation* probably uterine, and continuing either from the induced excited state of the nervous system, or perhaps, in the worst cases, from some lesion within the brain. *Treatment*: perseverance in the plans usually successful in convulsions; but chiefly counter-irritation of various kinds—sinapisms, blisters, &c., supposing bleeding to have been already practised.

ART. 157.—*Iodide of Potassium as an Anti-lactescant.* By M. ROUSSET.
(*Journ. de Méd. de Bordeaux*, May, 1858; and *Gaz. Hebd. de Méd. et Chir.*, Sept. 17, 1858.)

In cases where it is desirable to suppress the secretion of milk—inflamed breast or nipple, the birth of a dead child, and so on—M. Rousset has for some time been in the habit of using iodide of potassium in tolerably full doses. The anti-lactescant action, he tells us, is soon apparent. He appears to have had seven cases in which he has tried this treatment; but these are not reproduced in the ‘*Gazette Hebdomadaire*.’

ART. 158.—*Normal Lactation in the human race.*
By Dr. WM. HENRY CUMMING.

(*American Quarterly Journal of Medical Science*, July, 1858.)

In vigorous women the secretion of milk is copious; and this large amount is indicated in the unimpregnated state by the great development of the mammary glands. In no animal with which we are acquainted is there a larger promise in this respect. The amount ordinarily furnished by a good nurse is from one and a half to two quarts daily, or from four to five pounds; but cases often occur in which two children receive abundant supplies from one mother, involving a secretion of eight pounds at least. An infant three months old will take from forty-eight to sixty-four fluid ounces daily, in six or eight half-pint doses. During the first year, therefore, he will take from 1000 to 1300 lbs.

What is the composition of this milk? Without entering into long and tedious details, it may be simply said that, by the latest and apparently the most exact analysis, its composition is—

Butter 20·76	1000 lbs. therefore contain	Butter 20·76 lbs.	1300 lbs. therefore contain	Butter 27 lbs.
Casein 14·34		Casein 14·34 "		Casein 18½ "
Sugar 75·02		Sugar 75·02 "		Sugar 97½ "
Water 889·88		Water 889·88 "		Water 1157 "

In 1000 lbs. of milk there are 1·6238 lb. of salts, or 26 ounces, of which 0·5736 lb., or 9 ounces, are phosphate of lime.

In 1300 lbs. of milk the salts amount to 2·1 lbs., or 33½ ounces, of which 12 ounces are phosphate of lime.

It thus appears that, during the first year, the child receives from 110 to 143 pounds of dry solids. He may thus readily gain 15 or 20 pounds in weight (implying less than three pounds of dry solids), and yet have a large residue—from 107 to 140 pounds—to be expended in the production of heat, and in the activity of an energetic vitality. A child thus nourished can make teeth and bone without difficulty; his functional activity need never be suspended for want of material; atmospheric changes may be successfully resisted, and zymotic diseases will have little power.

And where, in the whole range of animal existence, will you find a more beautiful object than a vigorous, healthy child? Look at his deep and peaceful sleep; see the bright sunshine of a pleasant dream upon

his gentle face. Look at him as he awakes. Listen to the sweet sounds he makes for his own pleasure, or to attract the notice of those near. And if he should at last break out into loud cries, see how quickly all traces of sorrow pass away and bright smiles replace them, when his mother comes. See the eagerness with which he takes his food, the intense earnestness with which he clings to the abounding breast, and the full and deep satisfaction when this want is supplied. And when able to play, how loud and merry his laugh! How joyfully he receives caresses, or rides upon the knee, or springs in the arms of the parent or the nurse! There is no happier animal than a healthy child, nor is there anywhere to be found a more regularly operating and uninterrupted harmony of the vital functions. The beauty and energy of the outward frame are not more striking than the symmetrical development of the mental powers. The close and careful and patient observation, the cautious experiment, the unsuspecting credulity, the prying inquisitiveness, who has not beheld with admiration and delight?

This is infancy under the influence of favorable physical conditions; but, alas! how few children among us progress thus steadily and rapidly during their first two years! How commonly do we associate with infancy the ideas of sleeplessness and fretfulness, and all manner of gastric, intestinal, and nervous disorders! Why is it that "teething" does not mean the steady, silent, unnoticed development of the teeth, but salivation, and fever, and diarrhoea, and convulsions, and death? It is not pretended that there is never any other cause but insufficiency of proper food; but there is reason to believe that four fifths of the sufferings of infancy arise from this source. And how can it be otherwise? Look at the mothers, and say how many of them can give daily three or four pounds of good milk to their nurslings. How many can furnish 110 or 140 pounds of dry solids in the first year? How many can satisfy, can fully meet, the demands of even a feeble child? A *strong, vigorous, fat woman*, almost always loses weight while nursing her child. The milk draws away more than the stomach of even such a woman can replace, and the balance is taken by absorption from her previous accumulation.

And here listen to an important practical remark: a woman loses in an ordinary parturition not more than 20 pounds, containing less than 3 pounds of dry solids. This amount, furnished in nine months, is at the rate of 4 pounds of dry solids a year. Many women fail to furnish fully even this small amount. The infant at birth is small and meagre, looking like a starveling. Is it to be expected that such a mother will make a successful nurse? If unable to furnish this small amount, how can she supply thirty or forty times as much?

The truth is, that a woman, in fully nourishing her child, must furnish as much milk in proportion to her weight as a good cow. A woman weighing 130 pounds will give daily 4 pounds of milk, containing about 5 ounces of dry solids; the cow, weighing six times as much (780 pounds), will give 20 pounds of milk, containing 30 ounces of the same. It should not, then, surprise us that so many mothers fail to supply enough food for their infants. It requires great physical energy and powerful digestion to perform this work. How few mothers are thus endowed! If we may judge by the amount of food consumed by a vigorous woman

during the period of lactation, we should decide that the ordinary labour of a working man is less exhausting than the function we are considering. Certain it is that a vigorous woman, of strong digestion, while nursing a child, will eat largely, and yet lose weight.

The cases in which natural lactation fails are so numerous as to excite the deepest concern. Human milk can seldom be obtained, and none of the usually employed substitutes ordinarily succeed. Is it, then, too much to hope that physicians will give serious attention and thoughtful consideration to a plan offering a substitute for human milk, scientifically correct and practically successful?

ART. 159.—*Case of a pregnant Woman in whom both the mammae had been extirpated.* By ROBERT LEE, Obstetric-Physician to St. George's Hospital.

(*Medical Times and Gazette*, July 24, 1858.)

CASE.—September 9th, 1828.—A woman, about 40 years of age, in the last month of pregnancy, applied at the Westminster General Dispensary this morning for a midwife to attend her during her labour. She stated that both mammae had been removed for a cancerous affection several years ago, and that since their removal she had borne five children, which she had brought up by hand, and that they were all now in good health. Previous to the extirpation of the mammae, which was performed in Ireland, she had borne two children. She states that her mother and, I think, one of her sisters had been destroyed by cancer of the breast, and that she, becoming alarmed for herself in consequence of the appearance of hard painful tumours in both her mammae, consented to have them removed, which was accordingly done by a surgeon in Ireland. The glands had been completely removed, and the cicatrices were distinct. Since the operation she has enjoyed excellent health. During gestation nothing unusual is experienced in the situation of the breasts; but the second day after confinement there is an unusual fulness in the parts, and in the glands of the axillæ, similar to what occurs after delivery, when the secretion of milk is about to take place.

“I had nearly forgotten this case, when I received the following note from Sir Benjamin Brodie, which made me refer to my journal, where it had been recorded :

“‘Broome Park, near Dorking, Surrey ;

“‘September 29th, 1843.

“‘My dear Sir,—Will you be so kind as to inform me whether you have any experience as to what will happen to a woman who, having lost both her breasts, was delivered of a child? Would the constitution suffer from the entire loss of the apparatus for secreting milk under such circumstances?

“‘Believe me to be, dear sir, yours always truly,

“‘B. C. BRODIE.’

“In reply to this note, I inclosed a copy of the history of the preceding case from my journal. I have not since 1828 met with any other case like this.”

ART. 160.—*Starvation of a Child from paralysis of the facial nerve in consequence of a forceps operation.* By Dr. HOHL.

(*Monatschr. für Geburtskunde*, Oct., 1857.)

In the report of the lying-in hospital at Halle, a case is related in which the death of a new-born child was brought about in a very unusual manner. A woman, in whom labour was arrested through complete absence of uterine contraction, was delivered by the forceps, the head lying in the pelvic cavity. In consequence of the pressure of the left blade of the forceps, there resulted a paralysis of the facial nerve of the left side, which resisted all treatment. By this the infant was hindered from sucking, as all the milk ran out of the corner of the mouth on the paralysed side. Everything, prior to being swallowed, had to be placed within the grasp of the muscles of the pharynx. The child was thus gradually reduced by atrophy, and died on the twelfth day.

(B) CONCERNING THE DISEASES OF WOMEN.

ART. 161.—*On infra-mammary pain.* By Dr. COOTE.

(*Medical Times and Gazette*, July 10, 1858.)

The author remarks that pain immediately below the left breast, not of rheumatic origin, and unattended by any signs of visceral disease, has been recognised for about forty years as a very frequent, and often a severe and intractable, malady. Nevertheless, its pathology might be regarded as absolutely unknown, and its treatment was, of course, proportionately uncertain. It seemed, therefore, worth while to re-open the subject by the observation of fresh facts. He had, therefore, analysed a series of fifty cases, with the view of determining, firstly, the true characters of the pain, and, secondly, the conditions under which it was prone to occur. In the first place, it was necessary to distinguish between two painful affections, to both of which the infra-mammary region was liable, and the confusion of which seemed to account for many contradictions in earlier descriptions. The one (to which the name of intercostal neuralgia ought to be restricted) might affect any part of the thoracic walls. Its character was acute, plunging, paroxysmal. It was seated in one or more intercostal spaces, chiefly in those parts where the cutaneous branches of the nerves are most freely distributed, and it sometimes appeared to shoot round the chest, as if along the course of a nerve. There was occasionally much superficial tenderness, and the pain was sometimes periodical. The author thought it probable that the pain of herpes zoster and that of mastodynia belonged to this category. The other pain (improperly confounded with that just described) was much more common. It was a dull, aching pain, situated in one definite locality under the left breast, and extending, generally, over the seventh, eighth, and ninth ribs, with the seventh and eighth intercostal spaces; never appearing to shoot along the

course of a nerve, but often darting through the chest to the back or into the throat; in the former case, seeming to give rise to the inter-scapular pain; in the latter, being intimately associated with the hysterical globus. It was rarely marked by any considerable tenderness on pressure, and it was not periodical. It was to this affliction alone that the author wished to draw the attention of the society, under the (provisional) appellation of infra-mammary pain. Having discussed in detail each of the characters of the pain, he examined briefly the most popular hypotheses which had been devised to account for it. He dissented from Dr. Inman's view (that it is a true muscular pain, the exponent of fatigue or of mal-nutrition), on the ground that it entirely failed to explain the localization of the pain. There was no condition in the modes of life of the patients calculated specially to affect the pectoral and abdominal muscles of the left side. A more plausible hypothesis connected infra-mammary pain with uterine or ovarian disorder. Here a preliminary question arose, which the literature of the subject entirely failed to solve—whether the pain was peculiar to females? It was certainly of rare occurrence in the male; but the author thought he had noticed two unambiguous cases of it within the last eighteen months. Leaving this question open, and assuming, for argument's sake, that it was limited to females, he proceeded to inquire whether, in them, it was dependent upon uterine disorder. With respect to age, he found that the period of uterine activity was the favorite, but not the exclusive, epoch of the pain. Marriage exercised no perceptible influence upon it. Overlactation and excessive child-bearing were recognised in a few instances only. Four women were sterile; 7 had a liability to abortion. The menstrual function was physiologically absent in 20. Of the remaining 30, it was perfectly normal in 11; regular, but scanty, in 7; regular, but profuse, in 4; irregular or absent in 8. Leucorrhœa was acknowledged in 10 cases only, in 6 of which uterine disease existed. These facts appeared to be conclusive against the hypothesis. That uterine disorder frequently accompanied infra-mammary pain was certain; that it should be the cause of it was impossible; for those two things could not stand to each other in the relation of cause and effect, each of which might exist in the absence of the other. The next hypothesis was that of "spinal irritation." This term had been so stretched as to become meaningless, but the original idea differed very little from that of "central neuralgia." That infra-mammary pain depended upon some central (spinal) disorder, might or might not be true; but it was wholly unproved; and the attempt to prove it from spinal tenderness in such cases was doubly unfortunate. For, firstly, spinal tenderness was by no means a constant companion of infra-mammary pain; and, secondly, if it were so, it would be no evidence of spinal disease. The next hypotheses discussed were those of Ollivier and of Brown of Glasgow. Both agreed that the pain was the result of pressure upon the roots of spinal nerves; the former referring it to a congestion of the intra-vertebral plexus of veins, the latter to a transient curvature of the spine, occasioned by disproportioned fatigue of some one set of spinal muscles. These opinions were out of the sphere of argument. For, first, it was very doubtful whether such pressure would produce pain at all, and not rather anæsthesia; and, secondly, there was absolutely no

evidence of the existence of any such pressure. Another explanation, also based upon the idea of pressure, had been propounded by Henle; and this possessed the singular merit of recognising, and, in some measure, of accounting for, the localization of the pain. The anatomical character by which the left infra-mammary region was distinguished was the peculiarity of its venous circulation, the effect of which was that, if any obstruction existed to the return of the venous blood by the azygos vein, the brunt of the pressure would fall upon the intermediate intercostal spaces of the left side. Henle thought that such pressure, acting upon the peripheral extremities of the intercostal nerves, might occasion the pain; and he sought to dovetail his theory in with other received views, by suggesting that the first impulse to disturbance of the circulation might be given by uterine or ovarian congestion. There was little to object against this explanation, if the uterine element were eliminated from it, and the more physiological notion of interrupted nutrition were substituted for the mechanical idea of pressure. One link was, however, still wanting, viz., some proof that, in these cases, vascular disturbance exists. The author then gave the results of his own analysis. The constitutional character of the patients was well marked, being universally that of defective nutrition. Twenty-one were anæmic. The concurrent diseases were phthisis, secondary syphilis, and diabetes mellitus. The functional derangements accompanying infra-mammary pain were grouped under four heads. 1. Disorders of the nervous system, consisting of (*a*) various pains, of which the inter-scapular alone appeared to be essentially connected with the infra-mammary; and (*b*) spasms, especially the globus, and hysterical or epileptiform fits. In three instances these latter were always preceded by infra-mammary pain. 2. Disorders of circulation; variability of temperature, irregularity of the pulse, palpitation of the heart. 3. Derangement of the abdominal viscera; vomiting of porraceous or grumous matter, or of blood; constipation, or diarrhœa; the urine alternately "hysterical," and loaded with lithates. 4. Disorders of the reproductive system; uterine disease, leucorrhœa, irregular menstruation, sterility, abortion. The author proceeded to argue that the three latter groups might be readily referred to one head—disorder of the vaso-motory system of nerves. For that it was experimentally certain that paralysis of these, the motor nerves of the smallest arteries, had, as its immediate physical result, exalted temperature and local congestions and fluxes; and hence it seemed probable that to temporary depression of these nerves might be owing the irregular flushes, the porraceous or grumous vomiting, the deranged renal secretion, the ovarian and uterine disorders, so common in these cases. And as the muscles of the intestinal tube were supplied by nerves of the same order, the same hypothesis would explain the occurrence of obstinate constipation, associated as it is with hyperæmia of the mucous membrane. Hence he inferred, first, that infra-mammary pain was a symptom of a generally depressed state of nervous power; and, secondly, that it was one of a group of symptoms intimately connected with vaso-motory, and therefore with vascular, derangement; thus returning to the hypothesis proposed by Henle, and supplying the defective link. The conclusions drawn were as follows: True infra-mammary pain was a peripheral neuralgia,

having its probable origin in mal-nutrition of the nerves of the part. This, again, resulted from disordered circulation affecting the left infra-mammary region especially, by reason of its peculiar anatomical relations. The immediate cause of this vascular derangement consisted in disordered enervation of the smaller arteries of the whole body, occasioning irregular spasms and dilatation of their walls; a condition which, while in the infra-mammary region it occasioned neuralgia, in other parts gave rise to chills and flushes, to palpitation, to excessive or defective secretion, to congestions, hæmorrhages, and fluxes; while an analogous state of the motor nerves of the alimentary canal produced obstinate constipation. The cause of this disordered state of the vasomotor nerves was to be sought in more general conditions. The female, possessing naturally greater nervous irritability than the male, and physiologically destined to undergo great developmental changes, was far more liable to all these derangements, especially when suffering from want, or exhausting toil, or depressing or debilitating sickness. But there seemed no reason to deny the possibility of their occurrence, under analogous conditions, in the male. If these views were correct, the indications for treatment were twofold; first, to stimulate the vasomotor nerves into temporary activity, so as to relieve special symptoms; secondly, to give them permanent vigour by improving the general nutrition of the body. With respect to the first indication, the special nervine stimulant had often produced satisfactory, although temporary, results. Counter-irritation nearly always gave temporary relief, probably by unloading distended vessels. It was equally efficacious when applied to any part of the affected side. Topical applications to the vagina and uterus, in cases of leucorrhœa, &c., had produced no effect upon the pains. Sometimes the leucorrhœa was cured, leaving the pain as bad as ever; sometimes the pain disappeared, the leucorrhœa persisting. The second indication could be only briefly alluded to. Good food, air, above all, rest, were essential; and to them tonic medicines were merely auxiliary.

ART. 162.—*The right use of Sedatives in diseases of the Womb.*

By Dr. TILT, Physician to the Farringdon Dispensary.

(*Lancet*, May 22, 1858.)

“Pain is almost synonymous with disease, and is proverbially frequent, whether as the symptom of well-understood morbid conditions, or as the sole indication of disease. Pain can generally be cured by the systematic treatment of the attendant diseases, though it often specially indicates the exhibition of sedatives; and then the question arises whether they should be exhibited so as to act first on the system, or whether they should be directed to the seat of pain. Doubtless, a strong opiate, fully affecting the nervous system, has been known to remove the pain localized in some well-circumscribed spot, but in many cases it only procures a small abatement of the suffering, and the same dose cannot be repeated sufficiently often to cure neuralgia without locking up the internal secretions, and entailing a state of semi-narcotism very distressing to the patient. For these reasons, when sedatives are

used in neuralgic affections, they should be applied as near as possible to the seat of pain. We foment the painful spot with sedative fluids; we rub in sedative ointments and embrocations; we leave them on the skin, so as to envelop the extremities of the sentient nerves in a sedative atmosphere; we apply opiates to the denuded skin—they have even been injected with benefit into the tissues surrounding the agonized nerve. Although this principle is generally accepted as correct, it is not sufficiently followed in the treatment of diseases of women. I therefore propose showing what good results may be obtained by putting our practice in harmony with well-grounded theory.

“He must indeed be young in practice who is not aware how frequently pain and neuralgia, under various forms, appear as an important symptom of morbid menstruation, of diseases at the change of life, and of uterine and ovarian affections. Neuralgia in connexion with the reproductive system is still more frequent in women of the upper classes, in whom a nervous temperament has been inherited, and rendered more intense by injudicious schooling and by habits at variance with correct notions of hygiene. Suppose a patient suffering habitually from nervous uterine irritability, from bearing-down pains, vesical tenesmus, and severe pains in the sacrum and thighs, the necessity for sedatives will strike every one. Most medical men will give them by the mouth, either in such moderate doses that the patient’s sufferings will be long in abating, or, should the quantity have been sufficient to assuage the pain, the drugs may have acted so strongly on the system that it would take some days to recover from their poisonous effects. Afraid of this, others would apply sedatives to the pelvic regions or the loins; but before relief could be given much time must elapse, though, if the fomentations had been associated with sedative injections into the rectum, relief would soon be afforded, without any subsequent ill effects, because, the remedy being applied directly to the diseased nerve, the dose could be proportioned to the intensity of the pain. Is it not, then, better to give sedatives by the rectum than to leave a patient in an habitual state of suffering, or with the occasional variation of an over-dose of opium? This is applying the remedy to the right place, or *applying the sedative as near as possible to the suffering nerves*. In severe cases of uterine or ovarian neuralgia one ought not to prescribe in the dark. An accurate examination must be made, and none is accurate unless the eye can confirm the testimony of the finger. Often the neuralgic affection has been caused, or at least kept alive, by some slight ulceration of the neck of the womb, which can be seen but not felt; and when this is cured by surgical treatment, the neuralgia vanishes. At other times patients suffer greatly from abdominal neuralgia and slight uterine disease, which I have promised speedily to cure by cooling injections and slight applications of a strong solution of nitrate of silver at stated intervals. By these means I have soon cured the ulceration, but the patient, continuing to suffer as before, rings the changes on medical men, until cured by some more fortunate practitioner, or by time—the great physician. Such cases are not rare.

“Although advocating in no weak terms the use of sedatives, I in nowise repudiate what is advanced in my various works on the utility of surgical treatment in uterine affections; but only assert that uterine

pathology is not all included in ulceration of the neck of the womb, and that caustic is not the sum and substance of uterine therapeutics. In the milder forms of ovario-uterine neuralgia, as it attends morbid menstruation, diseases of the change of life, inflammatory and other diseases of the womb, I maintain that the neuralgic element should be attended to, although constitutional remedies and surgical treatment may be likewise required. The use of sedatives in such cases paves the way to more rapid cure, and I advisedly use the plural number, as it is better to exhibit a combination of several sedatives than one alone—a general principle of therapeutics well explained in the late Dr. Paris's 'Pharmacologia,' and which applies forcibly to diseases of women. In the cases above specified, I order a camphorated liniment, to the four ounces of which I add half an ounce of laudanum and two drachms of tincture of aconite. I recommend this to be rubbed carefully, for five minutes, on the lower part of the abdomen, or on the sacral region, or on both if they be the seat of pain. The application may be rubbed off; but the treatment will be more successful if left on, and a wadding poultice applied, and kept in place by a piece of oil-silk sufficiently large to wrap round the loins and fold over in front, thus enveloping the lower part of the abdomen in a sedative vapour-bath. Should this be ineffectual, I prescribe sedatives by the rectum, beginning with the following prescription: Battley's solution, one drachm; tincture of hyoscyamus, one ounce; spring water, three ounces. Half an ounce of this lotion contains one scruple of Battley's solution and one drachm of tincture of hyoscyamus, and I order this or double the quantity to be put into a two-ounce india-rubber bottle. This the patient fills up with warm milk, and after screwing on the canula, and anointing it with a little cold cream, it should be gently pushed up the bowel. When this is done, the firm pressure of the bottle by the hand will empty its contents into the bowel; the bottle should then be withdrawn; the mild nature of the fluid and its small bulk almost always allow of its being retained. This should be used twice a day; and if ineffectual, I add to the four-ounce solution one drachm of tincture of belladonna, or one or two scruples of tincture of aconite, or both quantities, which can be varied to meet the exigencies of different cases. These sedative solutions may be used likewise as vaginal injections, care being taken to so raise the pubis as to enable the fluid to be retained; but as this must be entrusted to the patient or nurse, it is impossible to know what quantity of the solution is placed in contact with the womb, and how long the contact is maintained. Therefore I deem it better to prescribe the sedatives by the rectum when their administration is to be entrusted to the patients themselves. When it is remembered that the reproductive organs are intimately connected with the lower part of the bowels by nerves, blood-vessels, and cellular tissue, one can understand that an excellent way of acting on the womb is by the rectum, the more so as its mucous membrane has a greater power of absorption than that of the vagina. I have dwelt on this at greater length in my work 'On Diseases of the Change of Life,' and shown that this mode of treatment is useful not only in the specified cases, but in cancerous affections of the womb, when it is necessary to abate the severity of pain without injudicious interference with the fragile tissues of the womb. This mode of treatment has even

a wider range than the removal of pain, for many obscure nervous affections may be thus amended, and mental affections may be thus cured when they depend on a perverted action of the reproductive system.

“One of the cases stated in the work just mentioned was a patient of my friend, Dr. J. Brown, of Chatham, who had for many months been troubled with strange delusions, and secluded herself from society. I took so serious a view of her case, that I told her husband my fear that it would soon be advisable to place her under restraint; but as there had been a great amount of uterine neuralgia ever since the cessation of menstruation, there was a chance that, by quelling this irritation, the nervous system might be calmed, and the mind strengthened. I therefore ordered strong sedative injections by the rectum, which were continued for some months, and were without doubt the principal means of restoring her to her previous state of good health.

“Such measures are generally found successful; but sometimes the patient will not or cannot retain the medicated fluid, and narcotic suppositories are objected to, or else the neuralgia may be too severe to yield to the treatment.

“Two years ago I was asked to see a lady residing a few miles from town. She was married, about thirty, of diminutive stature, nervous temperament, dark complexion, and reduced to extreme emaciation, having literally been confined to her bed for several months by an excruciating pain in the back and in the lower part of the abdomen. The pain was constant, but with paroxysms more violent at the menstrual periods. The flow was scanty, and there was a semi-purulent vaginal discharge. She had slept but little for many months, and had digested scarcely any food. The pain in the back was so intense that the late Mr. Lonsdale was consulted, and he is said to have called the case one of ‘hysterical spine.’ On examination, I found the womb exquisitely sensitive, its neck considerably swollen, and slight ulceration round the os uteri. Three leeches were applied to the womb; but the loss of blood they gave rise to, and the increase of pain they determined, prevented my repeating the application. I touched the ulcerated surface twice with the solid nitrate of silver, and afterwards with a strong solution at four days’ interval; cooling injections were used; and in a few weeks after the ulceration was healed, the uterine swelling had diminished, but the habitual pains had but little abated, and the rectal injections could not be retained by the patient. Such being the case, I placed one grain of acetate of morphine in a little cotton wool, folded it up, tied a piece of twine round it, and carefully placed it close to the neck of the womb. I then withdrew the speculum, leaving a piece of twine protruding, so that the patient might withdraw the little apparatus at the end of twenty-four hours, during which time no injections were to be made. Three days afterwards I applied two grains of the morphine salt in the same way; and four days afterwards, three grains. The day after this application there was a sudden abatement of the pains; and so great was the improvement, that the lady, who for months had only been able to crawl down stairs, to be carried back to her bed, was able to sit upright for several hours, and was not over fatigued by a two hours’ drive. This sudden amendment continued, so that in a few weeks she was able to leave for the sea-side.

"When I have had to do with submissive patients, I have never found this treatment fail in cases of uterine neuralgia, except once, when it was caused by pregnancy, and then, after trying various means, I resorted to this; but it brought on an alarming paroxysm of tenesmic pain, which subsided on the removal of the cotton wool. This patient continued to suffer until delivery, and I think pregnancy would not have gone its full time had I not found in the tincture of Indian hemp an admirable means of mitigating the severity of the pains.

"When the patient is married and the case severe, it is better to apply acetate of morphia carefully by means of the speculum and forceps. It may, however, be applied without using the speculum. If the forefinger of the right hand be introduced into the vagina in the usual way, along this finger the left hand can easily glide the forceps, armed with the cotton wool, until the neck of the womb is reached. When possible, I renew this application every second day. In this manner it is easily understood that the fluids in the vagina dissolve the acetate of morphia, and that the solution acts on the neuralgic womb. From the medical journals I find that Dr. Aran, of the Hôpital St. Antoine, has extensively followed the same idea. His plan is, to let fall one or two drachms of laudanum into the speculum previously introduced, fixing the fluid in the vicinity of the womb by a tablespoonful of powdered starch. Dr. Aran repeats the application every second day or every day, and has not seen the treatment followed by symptoms of narcotism. He has found it effectual in cases of uterine or ovarian neuralgia subsequent to inflammatory affections, or complicating uterine deviations. He has, like myself, found it useful in those singular and painful conditions of the womb which occur at the change of life. For the last six months, since I have been aware of Dr. Aran's plan, whenever I have to apply the speculum for the surgical treatment of uterine disease, I conclude by pouring into the tube a teaspoonful of Battley's solution and spoonful of starch powder, even when no pain is complained of, and it seems to promote the rapidity of the cure. I think, however, that my plan of applying the salts of morphia to the neck of the womb is preferable in obstinate cases of uterine neuralgia, like the one I have related. The question of priority is one of little importance to me; and Dr. Aran's testimony in favour of an idea which I have carried out for the last two years, made me think it worth while drawing attention to the right use of sedatives in uterine affections."

ART. 163.—*On exfoliation of the epithelial coat of the Vagina, producing casts of that canal; with remarks on the true form of the Vagina.*
By Dr. ARTHUR FARRE, Obstetric Physician to King's College Hospital.

(Dr. Beale's Archiv. of Med., No. II, 1858.)

In cases of dysmenorrhœa, as is now well known, certain portions or sometimes the whole of the natural lining of the uterus may be shed in the form of a dysmenorrhœal membrane. The identity of these membranes with the mucous lining of the uterus, as well as with the decidua

formed in early pregnancy, does not admit of dispute; and it is probable that a knowledge of this fact has led to the supposition, that all apparently membranous substances discharged under such circumstances, when not consisting of coagula or products of conception, are of this nature; but Dr. Farre has satisfied himself that some of these are not only materially different in structure from dysmenorrhœal membranes, but are not even of uterine origin.

This is the case with the three examples selected for the subject of the present communication.

The first occurred several years ago, in the case of a lady who came under my care for occasional attacks of dysmenorrhœa. Learning that she sometimes passed membranes, he procured one of these, and examined it carefully under fluid. The substance expelled consisted of a thin double layer, of a somewhat slight yet tough and parchment-like membrane, of an opake white colour, and smooth almost lustrous or pearly surface. It appeared at first to be a cast of the flattened interspace between the uterine walls which constitutes the cavity of that organ, and exhibited very much of the triangular form of the uterus. The two membranous layers of which it was composed were in close apposition, and were bounded by a peculiarly sharp, thin border, like the marginal folding of the uncut pages of a book.

Yet, supposing this to have been ejected from the uterus, there were still many conditions irreconcilable with such an hypothesis. The size of the entire cast, which exhibited the triangular outline of the uterus, was more considerable than the cavity of that organ in the usual unimpregnated state. The angles which should correspond with the points of entrance of the Fallopian tubes showed not the slightest trace of an aperture, and nowhere was there any of that cribriform appearance, produced by the pores of the uterine glands, which is so characteristically shown upon the inner surface of true dysmenorrhœal membranes, while the outer surface was not rough, but smooth and lustrous. The whole was found, upon microscopic examination, to consist of broad flattened nucleated cells of pavement-epithelium, and was entirely destitute of the histological characters of the uterine mucous membrane.

A preparation in the anatomical museum of King's College gave Dr. Farre the first clue to the true nature of these substances. The preparation is entitled, "False Membrane from the Uterus." Like the former example, it consists of a sheath of dense opake epithelium, but the outer surface, instead of being smooth, is indented everywhere, so as to form numerous pits and depressions, running in oblique lines, and exactly representing the course of the vaginal rugæ. When the preparation is laid open, and viewed from within, the furrows upon the reverse surface are seen to be converted into rugæ, having the ordinary arrangement of the columnæ rugarum upon the inner surface of the vagina. This specimen has more of the cylindric form figure than the former one, and at its upper end is a depression corresponding with the cervix uteri.

The chief differences between this and the former specimen are, that the cast is here evidently that of a narrower and more tubular canal, and exhibits the rugæ which are wanting in the former; differences which no doubt depend upon the circumstance that the one is the cast of the vagina

from an unmarried, and the other from a married person, in whom the surface of the vagina had become smooth by unfolding and obliteration of the rugæ.

A third example presented to Dr. Farre about eighteen months ago, by Mr. Henry Willington, of Brompton, has completely removed all doubts as to the true nature and source of these substances. As the history of this case is important, the author gives it in Mr. Willington's words:

"The 'mole' was passed by a married lady, at a menstrual period. She has borne three children; the youngest $4\frac{1}{2}$ years old. She was, when I first was called to attend her, the subject of severe pain, with sickness, at the menstrual periods, for which no relief had been afforded, and lately only, 'say four months before the passage of the mole,' she asked my aid. A few days previously to the menstrual period immediately before the one when the 'mole' was passed, she consulted me for a fulness at the anus, and great uneasiness in sitting down; accompanied with a peculiar movement of crawling in the vagina, 'up in her inside,' to use her own words. The sensation was intolerable, only relieved by an injection of Goulard water. The relief was complete for some hours. Not much notice was taken of this, until the bearing down and peculiar sensation came on again at the next menstrual period, and was described as a 'peculiarly crawling sensation.' An hour after I left the patient the proper menstrual discharge came on, and the mole was found in the linen, and was felt to pass the vagina. There was no hæmorrhage, nor increase of the flux, nor anything else that followed; nor in the three subsequent periods. The painful character of the menstruation is now much altered, and no drugs are now taken for it."

This specimen is in some respects more interesting than either of the two former. Its surface is smooth and shining like the first, and has the same dense, white, parchment appearance. It possesses the cylindri-form character of the second specimen, and at the same time exhibits at its upper extremity, in a marked manner, that peculiar crescentic border, perfectly destitute of any aperture at the apparent seat of the Fallopian tube (if this were a cast of the uterus), which was so puzzling a feature in the first specimen. There is here also, as in the other examples, an entire absence of the cribriform markings, and soft fleshy texture characteristic of ordinary dysmenorrhœal membranes. Like the other specimens, this cast consists of nothing but tessellated epithelium.

But the most important and interesting feature is observed in the upper part of this specimen. Here is seen a cup-like depression, having in its centre a transverse cleft, exactly corresponding in size and position with the two lips of the cervix, separated by the os uteri. So that in this case, not only the entire epithelial lining of the vagina, but that portion of epithelium also which covers the part of the cervix uteri that projects into the vagina, commonly termed the vaginal portion, has been exfoliated and expelled in one mass.

It is also interesting to observe that this process of desquamation has not in some of these cases been limited to a single act of exfoliation, but has been evidently repeated at intervals; for in two of the instances here given distinct traces of a second set of membranes were found enclosed within the first; and this fact illustrates, in a remarkable manner,

the statement made in the history of the last case, namely, that the "peculiar crawling sensation" experienced in the first attack, which was relieved by vaginal injections, came on again at the next menstrual period, when the two casts were expelled, one contained within the other.

The specimens here described are instructive in another and different point of view, as displaying the real form of the vagina, when in its ordinary state of vacuity and collapse. Being actual casts of that canal, they may help to correct the conventional notions of its form, which the ordinary representations in obstetric and other works are apt to give. For the vagina is not in its normal state an intestiniform tube of four or five inches in length, which is probably the general notion of it, although, from its great elasticity and capability of both elongation and lateral distension, it may be made to take various forms, so that it adapts itself alike to the ordinary tubular speculum, and to almost any form of pessary, whether globular, oval, or elipsoidal. In spirit preparations, also, as found in anatomical museums, an unnatural form is often given to the tube by distension of it before mounting; but if the vagina is examined *in situ*, just as Kohlrausch has represented it in his admirable sectional view of the female pelvis and its contents, of their natural size, it will be found to be a short flattened canal, the anterior and posterior walls of which are in mutual contact; measured along the anterior wall from the median tubercle of the vaginal orifice to the margin of the anterior lip of the cervix uteri, it commonly does not exceed *two*, or, at the utmost, *two and a half inches*, while the length of the posterior wall, from the hymen, or the entrance of the canal, to the extremity of the fornix, where the peculiar crescentic fold occurs, of which I have just given a description, does not ordinarily exceed *three inches*. The width ranges from one inch to an inch and a quarter, the broadest part being at the upper recess or fornix. The upper wall is shorter than the lower or posterior one, because the cervix uteri is let into it in a peculiar manner, close to its extremity, exactly in the position shown in the epithelial cast.

Dr. Farre believes that a knowledge of the several conditions under which exfoliation of the vaginal epithelium occurs, will be found to have an important practical bearing upon many of these abnormal conditions, not only of the vagina, but also of the cervix and os uteri, which so commonly fall under the notice of practitioners, and are accompanied often by so much local and constitutional irritation and disturbance.

The vagina deprived of its epithelium may be compared to the red and raw tongue of a person suffering from gastric and intestinal irritation. And it is probable that the extreme sensitiveness of the vaginal mucous membrane, which is so prominent and distressing a feature in erythematous conditions of this canal, is as much dependent upon an imperfect covering of the papillæ, occasioned by loss of their external epithelium, as it is upon a direct heightening of the sensibility of these structures.

ART. 164.—*On the use of gentian tents in partial occlusion of the Cervix Uteri.* By Dr. AVELING, of Sheffield.

(*Medical Times and Gazette*, June 26, 1858.)

The plan of treatment which Dr. Aveling proposes is this: "A piece of gentian root is easily obtained, and any one possessing a penknife

can manufacture it into a tent of the required size. It may readily be introduced by the aid of a pair of common dressing forceps, without using the speculum. A piece of string should be tied to its vaginal extremity, for the purpose of removing it after it is expanded. In less than four-and-twenty hours the tent, by the absorption of the fluid with which it is in contact, will have dilated the canal as far as it is able. It may then be withdrawn, and another introduced of a larger size if necessary. In treating these cases, it must be remembered that the seat of stricture may be nearly two inches higher up than the os tinea. The tents should therefore be two inches and a half long, at least, so as to ensure their penetrating the os internum. Dr. Aveling has been struck with these tents coming out marked deeply by the strictures. In one case, in which the stricture had been caused by the application of powerful caustics, the tent appeared as if it had been tightly constricted by a piece of fine cord. The discharge during the presence of the tent is, of course, of a brown colour. Injections of warm water should be frequently used.

Should there be a tendency in the strictures, after they have been dilated, again to contract, one of Professor's Simpson's stem pessaries should be worn.

The gentian tent is cheap, simple, and efficacious. No other requirements can be sought for in an instrument."

ART. 165.—*On the beneficial effects of Pregnancy and Rest in the recumbent position upon Retroversion and Prolapsus of the Womb.* By the late M. BRACHET, of Lyons.

(*Gaz. Méd. de Lyons* ; and *Gaz. Méd. de Paris*, May 29, 1858.)

M. Brachet relates three cases in illustration of this plan :

CASE 1.—The womb of this patient became retroverted after her fourth confinement, and for thirteen years this complaint had rendered her a complete invalid. All the medical resources of Lyons and Paris had been tried without avail. In 1847 she once more became pregnant, and in due time was delivered without any difficulty or accident, after which she was ordered by M. Brachet to remain in bed for forty days. Fifteen days after her confinement she was ordered, in addition, to use injections of decoction of oak bark, and to introduce in the interval small portions of sponge smeared with an ointment containing tannin. At the end of the forty days the patient was able to get up without the return of her old symptoms, and, since this time, although by no means leading an idle life, she has continued well.

CASE 2.—This patient became afflicted with complete prolapse of the womb after her third confinement, and for two years the ordinary means had failed to produce any relief. Becoming pregnant again, M. Brachet advised her to remain in bed for forty days after her confinement, and with the same result as in the last case. This was in 1851. Since this time, moreover, she has had another child without any return of her former infirmity.

CASE 3.—This patient became afflicted with prolapse of the uterus after her fourth confinement, and for some time she wore a pessary. Again she became pregnant, and her delivery took place in 1855. Forty days in bed after this event, as in the two former cases, was productive of a complete cure.

ART. 166.—*Inversion of the Uterus reduced after six months.*
By Dr. WHITE, Professor of Medicine, &c., in the University of Buffalo.

(*American Journal of Medical Science*, July, 1858.)

CASE.—Mrs. —, æt. 30, was taken in labour, at the full term, for the second time. This labour was natural to the conclusion of the second stage, when she gave birth to a large male child. Placenta adherent, but removed at the expiration of about thirty minutes, and its delivery followed by copious flowing, severe pain, and faintings. The prostration was so great as to require the constant use of stimulants during the succeeding forty-eight hours, and for three weeks she continued extremely weak and faint. At about this time she took an aloetic cathartic, which occasioned violent efforts at stool, accompanied by pains resembling those of labour. Profuse hæmorrhage followed these straining efforts, and a large pear-shaped tumour made its appearance through the os externum. The neck or smaller extremity of this body was at the vulva, and the larger extremity down between her thighs. By the assistance of her husband, she was enabled to return this tumour within the vulva, when a messenger was despatched for Dr. Batten. Dr. Batten, upon his arrival, introduced his hand into the vagina and carried the uterus high up into the pelvis, and resorted to astringents and cold for the purpose of arresting the flow of blood, which continued profuse and difficult to control. The prostration being at this time very great, the horizontal posture was enjoined, stimulants and tonics given, and the bowels moved by enema.

During the succeeding three months she had occasional hæmorrhages, which were severe, with constant discharge of muco-sanguinolent matter. Two or three times during this period she so far improved as to walk about her room, and partially supervise her domestic affairs, though looking very pale and being very feeble. About the middle of January she had another severe attack of hæmorrhage, the tumour again presented externally, and was again returned as before; that is, pushed back within the vulva. Dr. Batten again visited her, and prescribed such remedies as seemed necessary to control the flowing. Since about the 1st of February she has been compelled, from the debility consequent upon the exhausting sanguinolent and leucorrhœal discharges, to preserve the recumbent posture. Lactation, doubtless, added to the exhaustion, and being confined to her bed she had little appetite, the stomach was irritable, and the bowels costive. Ever since the patient took the aloetic cathartic and the tumour made its first appearance between the thighs, she has been aware of the existence of something unnatural in the vagina. This body has occasionally made its appearance externally, requiring the assistance of her husband to replace it, and she has had frequent attacks of a "straining sensation" described as accompanying its first complete descent. She has suffered greatly from all the symptoms arising from exhaustion and sympathy with the uterine irritation necessarily developed by its malposition. The pulse now numbers 130; she is blanched or wax-coloured, and grows dizzy and faint when raised to the semi-recumbent posture, and cannot be moved without producing a sense of prostration. It should have been stated, that on the 25th of February Dr. Robinson, of Hornellsville, and Dr. Reynale, of Downsville, visited the patient in consultation with Dr. Batten, when inversion of the uterus was diagnosed, and measures were resorted to calculated to ameliorate her condition.

Upon making a careful examination (nearly twenty-five weeks having now elapsed since confinement), the fundus of the uterus is found just within the os externum, and about one inch and three fourths in its transverse diameter,

and scarcely exceeding an inch in its antero-posterior diameter. The body and neck of the organ occupy the vagina, and the neck is not more than one inch in diameter, and feeling like the pedicle of a polypus. The inversion is recognised as complete, and the organ scarcely, if at all, larger than when in its natural position six months after delivery. Introducing a large cylindrical speculum into the vagina, the fundus of the uterus passes readily into its cavity, thus demonstrating the complete involution of the uterus, and bringing distinctly into view the rough mucous membrane of its now outer covering, which bleeds upon the slightest touch with the finger or sound. It is seen to be covered with muco-purulent matter also, and not susceptible of indentation by pressure with the point of the sound.

The bowels having been freely moved by an enema, I proceeded to the operation of reduction, in the presence of Drs. Robinson, Reynale, Batten, Dimick, and Mr. J. W. Robinson, medical student. The patient was placed for the operation, as before, upon an elevated, firm bed, with the hips brought quite to its edge, the knees separated, the feet resting in the laps of Drs. Reynale and Robinson, with directions to each to support a knee and hand of the patient, and prevent her from moving about. Dr. Batten brought the patient moderately under the influence of chloroform, which was continued throughout the operation, whilst I placed myself upon my knees, between the limbs of the patient, her pelvis being at a convenient elevation for manipulation. I introduced my right hand completely into the vagina, and firmly grasped the entire body and neck of the uterus. It may here be remarked, that the parts were so firmly contracted as to render the introduction of the hand difficult. At the same time that the entire uterine tumour was grasped by the right hand, the large rectum bougie described in the first operation was carried up, and also received into its palm, and held firmly in contact with the fundus of the uterus, the hand being sufficiently large to receive both, and keep them in apposition. Continuous, gentle pressure, was now made upon the external extremity of the bougie with the left hand, whilst the right compressed the uterine tumour, and kept the upper extremity of the instrument directly upon the fundus, and with the dorsum of the hand in the concavity of the sacrum, directed the force in the axis of the pelvic cavity, putting the vagina completely upon the stretch. This pressure was exerted, and this position unintermittingly maintained, although the force was not to such a degree as to endanger laceration of the utero-vaginal connexion, until my strength was nearly exhausted from continuity of effort. At length, and when about to relinquish the task, the uterine tumour began to shorten *at its neck*, and the mouth of the organ to push upon the upper surface of the hand. No depression or dimpling of the fundus was at any time perceptible. Ascending more and more rapidly as the neck diminished in length, the fundus finally passed out of the hand, and was easily pushed by the bougie through the mouth and neck of the organ up to its proper position.

In order to verify the restoration, Simpson's sound was introduced alongside of the bougie, and was found to enter a little more than two inches and a half above the os, which could now be distinctly felt. The large speculum, already referred to, was now slipped up around the bougie, when the os was brought distinctly into view, surrounding also the bougie. The sound was again carried through the os to the fundus, through the speculum, and all the medical gentlemen present saw that it passed easily beyond the mouth to the shoulder of the sound, and could not, without force, be carried further. Thus was demonstrated not only the reduction of the uterus, but that the organ was accurately measured, and found scarcely, if at all, enlarged. The speculum

and sound were now withdrawn, the patient carefully removed to the bed, and the bougie retained in place by the hand, to prevent reinversion. Meanwhile, stimulants were given to sustain the patient, and ergot in such doses as were deemed likely to excite the tonic contraction of the uterus. The patient soon recovered from the effects of the chloroform, and expressed herself as feeling quite as comfortable as before the operation. The patient suffered but little during the operation. The discharge of blood was slight, and when the effects of the chloroform had passed off, and she had taken a little brandy and water, she expressed herself as feeling comfortable. Pulse not sensibly changed in quality, and numbering the same as before the operation.

Drs. Robinson and Batten remained with the patient during the succeeding night, alternately maintaining the bougie in the uterus, supporting it gently, and rendering such other attention as the patient required. Continuing the pressure upon the fundus of the uterus was perhaps unnecessary, but it was thought safe not to withdraw the bougie until the next day.

March 13th.—Dr. Robinson writes: "The patient is quite comfortable; pulse 108. Free from pain. Withdrew the bougie this morning, and found the os uteri embracing it pretty firmly."

Tonics, with nutritious diet, were continued, and quietude in the horizontal position enjoined.

On the 15th, he writes: The patient is quite comfortable this morning. Made a digital examination, and found the uterus perfectly *in situ*, and mouth well contracted. Has some appetite. Pulse 100."

March 23d, he again writes: "She is improving; has been much annoyed by neuralgia and sickness of stomach, but both are giving way, as is the leucorrhœal discharge. The treatment has been sustaining (porter, wine, quinine, iron, &c.), with nutritious diet." In conclusion, Dr. Robinson adds: "She will get well, and I feel gratified in the success of the effort of restoration, not only on account of the patient being relieved of a loathsome malady, but also that I have been instrumental in contributing to the professional reputation of one for whom I feel a deep friendship, as your success in this case will relieve obstetrical surgery from the opprobrium which has hitherto been attached to it in cases of chronic inversion of the uterus."

In reply to a letter of inquiry from me, Dr. Robinson writes, April 22d: "I visited her yesterday, and found her very cheerful, and able to sit at the table and take tea with us. Her final recovery is now beyond all doubt. In short, the operation has been as successful as its most sanguine friends could desire. Her convalescence has been protracted; slow, perhaps, but when we take into account the great prostration from the long continuance of the malady, and the exhausting hæmorrhages and leucorrhœal discharges to which she had been subjected, the only wonder is that she recovers at all."

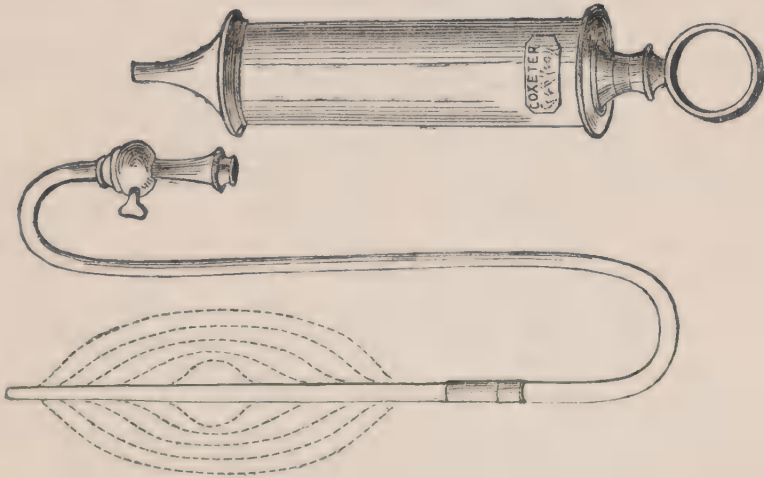
ART. 167.—*On dilatation of the Female Urethra by fluid pressure.*
By Mr. T. SPENCER WELLS, Surgeon to the Samaritan Hospital, &c.

(*Medical Times and Gazette*, July 24, 1858.)

"Towards the end of last year," says Mr. Spencer Wells, "Mr. Henry Thompson showed me a modification of Dr. Arnott's instrument which he had contrived for the purpose of compressing the prostate by dilating the prostatic portion of the urethra. I at once saw that a very slight modification of this instrument was exactly what was wanted for the female urethra; but it was not until last month that I had need of such an instrument. I was then consulted by a lady in whose bladder

I detected a stone. In an ordinary case of the kind I should have crushed it at once, but in this case there was so much irritability of bladder, the urine was so loaded with mucus, and so soon became ammoniacal, that I was doubtful whether it would not be wiser to run the risk of incontinence after dilatation, than increase the irritability of the bladder by the use of the lithotrite, and the escape of the fragments of stone. I therefore advised a consultation with Sir Benjamin Brodie, and acted on his advice, which was to inject the bladder with water daily for a few days, and then break the stone, being prepared at any time to dilate the urethra and empty the bladder, should any undue irritation come on. So far all has gone on well. The irritation diminished directly the stone was broken, and very little is left of it after three sittings.

“But the instrument I had prepared for this case by Mr. Coxeter I have had an opportunity of trying in another. The instrument is shown in the accompanying woodcut, the dark line representing the instrument before, and the dotted lines after, dilatation.



“It consists of a female catheter, a piece of india-rubber tubing fitting closely over the catheter, an elastic tube furnished with a stop-cock, and a syringe. On filling the syringe with water, fixing it to the end of the elastic tube, and injecting the water from the syringe into the catheter, the water is forced through small openings near the end of the catheter, and distends the india-rubber tubing which covers it. When the syringe is emptied the stop-cock can be turned, and the syringe refilled. The india-rubber dilates, as shown by the dotted lines, at first in a globular form, afterwards in a more oblong direction, especially if any lateral pressure be made on it. In this way the urethra may be very gradually dilated with an equal pressure in every direction until its diameter exceeds an inch. The length of the elastic tube permits of the use of the instrument beneath the bed-clothes without the least exposure of the patient.

“It is hardly necessary to describe the mode of using it, but I may say that the covered catheter is oiled or covered with some greasy substance. The only thing in common use which appears to spoil the india-rubber is

turpentine. When oiled it is introduced as an ordinary catheter, and so held that the centre of the distending portion is kept just within the meatus. There is some little tendency in the bladder to force the instrument outwards, but slight gentle pressure easily overcomes this. After one syringefull of water has been gradually injected, a second may be used, and a third if very great dilatation be required. On ceasing the injection the distended tube is at first held very tightly—at least it was in the only case I have had—but after a minute or two it may be moved and easily withdrawn. The fore-finger, oiled, can then be inserted at once into the bladder, and followed by any instrument of equal size.

“The effect was admirable in my patient. There were reasons against giving chloroform or ether, so that I was able to judge of the rapidity and painlessness of the process as compared with the sponge-tent. There was some pain, but it was by no means excessive, and the dilatation did not occupy more than ten minutes, yet I was able to introduce a pair of bullet forceps, and remove an oblong foreign body with great ease, and after the third day the retentive power of the bladder has been perfect.

“I intend to take the first opportunity that occurs in the dissecting room of testing the power of a similar instrument on the male urethra, after opening it as for lithotomy. It may not be generally known that lithotomy—or rather lithectasy—on the horse is performed on the principle of dilating the prostatic urethra. The staff is passed along the urethra, an opening is made into its membranous portion from the perinæum, and an empty bladder is then carried along the groove of the staff into the horse’s bladder. The staff is withdrawn, and the prostatic urethra dilated by injecting the introduced bladder. Sufficient dilatation can be obtained in this way for the removal of very large stones; and I think all that we know of lithectasy in the human male leads to the conclusion that we only want a more perfect means of dilating the prostatic urethra to do away with the most formidable dangers of lithotomy. Some such instrument as that which has answered so well for the female urethra will, I trust, prove useful in this way.”

ART. 168.—*On the successful treatment of Vesico-Vaginal Fistula.*

By Mr. J. BAKER BROWN.

(Pamphlet, 1858.)

In this memoir, which was read before the British Medical Association at Edinburgh, on the 31st of July, 1858, Mr. Brown gives the result of his ample experience in the treatment of vesico-vaginal fistula. Since our last notice of Mr. Brown’s labours (*v.* ‘Abstract,’ XXVII, 224) this gentleman has had seven cases, all treated by Dr. Bozeman’s method, and all successfully. These cases call for little remark. They prove, conclusively, that this intractable lesion is now quite under our control, and that no case need be rejected as incurable. Cases 1, 2, 4, 5, 7, 9, and 10, were cured at the first operation; cases 6, 8, and 11, at the second; case 3 was operated upon nine times before the cure was accomplished—a fact which gives us great encouragement to persevere, even in the most difficult and obstinate cases.

A remark is added, in conclusion, which assigns the credit of having first used metal sutures to Mr. Gossett, of London, and of having perfected the operation to Dr. Bozeman.

"The evening previous to my reading these cases before the British Medical Association at Edinburgh," says Mr. Brown, "my friend, Professor Simpson, informed me that Mr. Gossett, of the city of London, had published, in the 'Lancet' for 1834, the history of a case of vesico-vaginal fistula which he had cured by using golden sutures, and that he also recommended their use in many other surgical cases. The merit of being the first to apply metallic sutures to these cases is therefore undoubtedly due to Mr. Gossett of London, and not to Professor Sims of New York. I must further add, however, that I attribute the rapid success of these operations to the use of the button, as first suggested by Dr. Bozeman, and that to him therefore is fairly due the merit of rendering this most troublesome lesion comparatively easy of cure."

ART. 169.—*An analysis of 61 cases of Ovariectomy.*

By Dr. GUSTAV SIMON.

(*Scanzoni, Beitrage zur Geburtsh. ; and New York Journal of Med.*, July, 1858.)

This review of operations performed by German surgeons has been prepared with a great deal of care and judgment, in order to establish the true value of the operation. It contains not only those cases which have been published previously, but besides, 23 cases are added which were communicated to the author by private letters, mostly from the operators themselves. In reporting each case, he does not confine himself to short notices, but gives a history of every one of them. The results of the operations are divided under three heads, viz., 1. Operations followed by a radical cure. 2. Operations resulting in death. 3. Operations from which the patients recovered at first, the ultimate good result of which was only temporary, dubious, or of no consequence at all. In the latter division are counted those cases where the operation had to be given up in consequence of too strong adhesions or a wrong diagnosis, or where the patients died at a later period from the operation, or from the original disease.

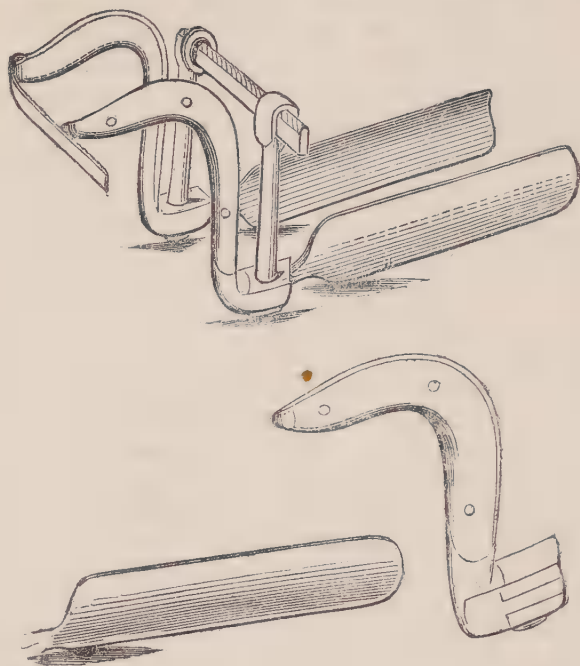
In all the former statistics (American, French, English) the cases coming under No. 3 are reported among the successful operations. Dr. Simon's analysis should therefore claim the undivided attention of the profession. Moreover, as most of the operations were performed at a recent date and by eminent surgeons, good diagnosis, scientific performance of the operation, and skilful after-treatment, were commonly secured.

Results : Of 61 patients operated upon, 44 died immediately after the operation, or $72\frac{8}{61}$ per cent. In 5 patients the operation was of transient or no benefit at all, and only 12 were radically cured, or $19\frac{4}{61}$ per cent. These results are by far less favorable than those of former statistics, and, from a comparison, ovariectomy is more dangerous than the Cæsarian section, because only 63 per cent. died in consequence of the latter, according to Dr. Kaiser's statistics, or about $\frac{2}{3}$ per cent., according to some other authors.

ART. 170.—*A new Speculum Vaginæ.* By Dr. ROBERT NELSON.

(Lancet, Feb. 28, 1858.)

The instrument which is represented in the accompanying cut consists of a *potence*, movable handles, and removable blades; it is used with the handle upward, and not downward, as that of Madame Boivin.



The left pillar of the *potence* is a fixture to the upper bar, flat on each side, and a quick thread, chased on the upper and lower side, to which a milled nut is adapted. The right pillar fits to, and rides easily on, the bar, so that by a few turns of the nut the pillars can be separated to any extent of distension that the vulva is capable of; after which, by pressing the horns of the handle together, the uterine ends of the blades are made to expand to the full extent of the fundus of the vagina, rendering it tense, if needed for cutting. The vulvar extremity admits of insinuating the finger and instrument during an operation, and their free movement in any direction, deeply as to the os *tineæ*, or as superficially as to the nearest part of the vagina. There is room for the assistant to seize with a forceps and hold any part without embarrassing the operator by the presence of his hand, one working between the blades above, and the other below.

The best shape for the bladder is quite flat, with the edges slightly bent inward. It must be made of steel, very thin, and electro-plated. Spare blades of various sizes, breadth or length, and of any shape, to suit a particular case, may be added.

ART. 171.—*A case of premature Puberty.*

By Mr. R. B. SMART, of Manchester.

(Proc. of Roy. Med. and Chir. Soc., June 22, 1858.)

CASE.—Mary D—, a girl born in Manchester in January, 1853, had an appearance of hair on the pubes, followed by a menstrual discharge, at the age of three years and six months. None of the assigned exciting causes of early puberty could be traced. She seemed to be generally out of health before the first occurrence of the catamenia, but has since become strong and well, the menses recurring with perfect regularity up to the time of observation. She is now (May, 1857) three feet seven inches in height, and weighs fifty-two pounds. Her general appearance is wonderfully like that of an adult female of short stature; her bust is full and womanly; the breasts large and prominent, with the characteristic glandular feel on manipulation; the nipples are well developed, and the veins of the breasts much marked. Rather from the abundance of fat over all the body than from the size of the bones, she is wider across the hips than other children; she has a distinct forward curve in the lumbar spine; the hair on the pubes is light brown, and about an inch in length: the external parts of generation have very much the conformation of an adult. In intellect the child is backward; her disposition is lively, but obstinate. There is no positive indication of sexual propensities.

The case has a table of measurements, in which the proportions of this child are contrasted with those of her sister, who is a year and eight months older than herself. This sister is shorter, and weighs nearly a stone less than the subject of the paper, and many of the measurements are in excess in the younger girl, especially the girth of the body and limbs. Photographic portraits of the case represent at a glance the chief peculiarities. The writer of the paper has given a table of references to other similar cases published in England, and makes some comments on the points which are of interest to the physiologist.

(C) CONCERNING THE DISEASES OF CHILDREN.

ART. 172.—*Malarious remittent Fever in children.*

By Dr. C. HANDFIELD JONES, Physician to St. Mary's Hospital.

(Brit. Med. Journal, July 31, 1858.)

There is a form of febrile disorder occasionally met with in children which does not correspond to the description given by our best authorities of remittent fever, and is further differentiated by the effect of remedies.

A child of two to six years old (I sketch the limits, of which I have notes) has been gradually losing health for several days or weeks; it is languid, drooping, emaciating, and, without appetite, more or less thirsty. The night is the time of most disorder; there is considerable cerebral disturbance, quite amounting to delirium in some cases, in others, marked by "horrible dreams," talking in sleep, and great restlessness. In some there is at night also copious perspiration. The child may be

apyretic during the day, but becomes feverish at night. The bowels may be costive or much relaxed. The tongue is clean in some, in others coated. In these cases, I have found quinine of decided curative efficacy, improvement taking place under its use in a way that could not be referred to the unaided *vis medicatrix*. A child of five years old has taken eight, and another of four years has taken six grains daily, with great benefit. Cod-liver oil and steel wine may sometimes be given also with advantage.

I am much inclined to consider these fevers as of malarious character, using the term in a rather wide sense, as applicable not only to marsh miasmata, but to other ill-defined poisons, which may be generated even in cities. I do not think the disease in question is typhoid fever; it does not appear to tend to a spontaneous termination, or to have a definite period. Its pathological affinities, as far as I can judge, are in the direction of neuralgia and dysentery or diarrhoea.

ART. 173.—*On vesicular Emphysema of the Lungs in early childhood.*

By Dr. GRAILY HEWITT, Physician to the Samaritan Free Hospital.

(Pamphlet, Churchill, 1858.)

Vesicular emphysema of the lungs, as occurring in the adult, has received a large share of the attention of pathologists; and whatever may be said of our knowledge respecting the mode of its production and of its origin, we are at all events well acquainted with many of its effects, and in possession of means enabling us to recognise its presence. Not so, however, in the case of children. Emphysema is chiefly known in what may be termed its mature state, or last stage—that in which the air-cells have attained a large size. In children we have the disease presented to us in its earliest condition—that in which, consequently, it is most advantageous to study it. No morbid lesion of the lungs is indeed more common in early life than emphysema—a fact which, for many reasons, deserves to be more widely known than appears to be the case at present. The reason that emphysema in childhood is so little alluded to by authors, and so little practically taken into consideration by practitioners, would seem to be, that, at this period of life, the enlargement presented by the air-cells is, comparatively speaking, not great in degree, so far as the individual cells are concerned; and that, unless their size be specially regarded, the slight enlargement is liable to escape observation after death.

That emphysema of the lungs most undoubtedly plays a very important part in most of the diseases of the respiratory organs in children, that it adds no little to the difficulties encountered by both patient and physician in combating and overcoming their exceedingly fatal tendency, and that it has probably no small influence in hastening the fatal event in some diseases, in which its existence is hardly suspected—these are circumstances to which Dr. Hewitt desires to call particular attention. In cases where recovery from these affections takes place, the lungs are often left permanently injured. The adult, tormented by chronic bronchitis and emphysema, is not rarely found, on inquiry, to have been the subject of respiratory anomalies of various kinds from an early period of

life; the disease of which he was supposed to have been cured when in the nurse's arms has left behind it a malady which, slowly developing and increasing with advancing years, has, by impairing, though perhaps slightly, the physical activity of the individual, exercised no slight influence on the whole tone of his life and character, and paved the way to other and serious organic diseases.

"The chief generalization," says Dr. Hewitt, "which the facts in my possession warrant me in making is, that although no part of the lung-surface seems absolutely free from the liability to be affected with emphysema, yet that it is rare to meet with it in its primary and simplest form at the extreme borders of the lobes—at those parts, in fact, which are the most distant from the bronchi. These latter situations are, indeed, in children, the particular seat of apneumatosi*s*—a condition which is in its nature the very opposite of emphysema.

"The most usual form in which emphysema occurs in the lungs of children is the following: slightly elevated whitish patches or islets are seen scattered over the surface of the lobes, abruptly separated from the adjacent lobules, which are either healthy or affected with apneumatosi*s*, and the air-cells themselves variously enlarged; this intermixture of emphysema and apneumatosi*s* gives the surface of the lung an uneven, pitted appearance—very characteristic. The emphysema is not often confined to one lobe. To the casual observer, the more evident pathological change in by far the majority of these cases of emphysema—a pathological change with which the emphysema appears to be inseparably connected—is the collapse of certain of the lobules of the lung, produced by withdrawal of the gaseous contents of the air-vesicles. So intimate, indeed, is the connexion between the two lesions—emphysema and apneumatosi*s*—that a description of the one necessarily includes a reference to the other. I have never seen apneumatosi*s* unaccompanied by emphysema; and if large portions of the surface of the lobe present the one lesion, it is the rule that adjacent portions of the surface are affected with the other. Solidification of the lung-tissue, due to the presence of tubercle or true pneumonic inflammation, appears, though in a less degree and less constantly than is the case with apneumatosi*s*, to entail with it an emphysematous change in the vesicles of neighbouring lobules.

"It has been already remarked, that the individual air-cells are not found greatly enlarged in size. The average measurement observed was about one fiftieth of an inch in diameter, this being about twice the diameter of the healthy air-cells—according, at least, to the standard I have been induced to adopt. Instances were not rare in which the enlargement reached one twentieth or even one tenth of an inch; but in these cases, only two or three cells out of a series of ten or twenty presented emphysema in this degree."

Speaking of the causes of emphysema, Dr. Hewitt says:

"Whatever reduces the bulk of the lung at any one point, as a secondary consequence produces emphysema. Keeping this principle in view, it is easy to explain the appearances presented to us in the lungs of young children dying of lung-affections. It will be profitable to consider, in the next place, the comparative frequency with which the various bronchitic and other affections incidental to young children become com-

plicated by emphysema, and to assign to the latter condition its due importance as the pathological cause of many of the symptoms observable during life.

"Amongst the first, chronologically speaking, of the affections of the lungs to which infants are liable, and which are concerned in the production of emphysema, is atelactasis, or non-aëration of certain pulmonary lobules after birth. Children in whom large portions of the lungs do not expand after birth rarely survive many days; but when a less degree of the disease is present, life may be prolonged for a variable period. In this latter class of cases, when death takes place, emphysema is always found. Under these circumstances, however, the enlargement of the air-cells is not so great in degree as that produced by other pathological conditions; the walls of the thorax are less resistant than at a later period of childhood, and more readily adapt themselves to the shape and size of the lung. In cases which ultimately recover, the lung often remains permanently injured; the portions of lung at first healthy, and subsequently unduly distended, owing to the presence of atelactasis, do not regain their natural dimension, and the child is left to breathe habitually under disadvantageous circumstances, its *respiratory surface* having been diminished in extent. The puny child grows into the stunted adult, the developmental energy of the various organs being thus early impaired and weakened. It is a question whether nature is capable of remedying the deficiency in the extent of respiratory surface, by producing new air-cells in the interstices of the old ones. A certain lobule, we will suppose, never expands, is atelectatic from birth. Pathology teaches us that this lobule diminishes in bulk, and is probably absorbed. The surrounding lung-tissue, which at birth was normally expanded, takes the place in the thorax of the non-expanded lobule. It appears probable, judging from pathological data, that the space is filled up by the abnormal expansion of the air-cells already existing, rather than that there is a new creation of air-cells. The reason that emphysema produced by atelectasis is slight in degree is, that the shape and size of the thorax is from the first regulated by the degree of expansion of the lung-tissue.

"During the first two years of infant life, bronchitic affections are exceedingly frequent, and assume an importance which can hardly be exaggerated. Why do these diseases so frequently kill? How is it that the mortality from these affections is of a degree absolutely startling to those accustomed to deal with the statistics of infantile disease? These questions must be interesting to all. There are few diseases which suddenly, and by the introduction of one lesion alone, bring about a fatal result. The more common circumstance is, that the primary affection produces a second, the second a third, and so on.

"Nature interposes, sometimes effectually, and the exaggerated functional activity of one part of an organ is made to compensate for the diminished or arrested activity of another. This compensation, successful for the moment in affording relief, has, however, often the effect of deteriorating the previously sound and healthy, and otherwise unaffected portion of the organ attacked.

"Now, the series of changes found in the lungs of children dying from bronchitic affections illustrate in a very forcible and apposite manner the

truth of the generalization here enunciated. The first effect of bronchitis or catarrh in an infant is obstruction to the entry of air. This obstruction increasing, certain portions of the lung become apneumatic—return to the quasi-fœtal state, as in the case of atelectasis just alluded to, but in a greater degree. The adjoining healthy lobules take on a compensating action; but by so doing, themselves become diseased—in fact, emphysematous. The act of inspiration forces the lungs to fill out the cavity of the thorax; but there being an impediment to the entry of air in the bronchi leading to the apneumatic lobules, it naturally finds its way into those not so obstructed. The emphysema thus produced is often extreme as regards the amount of lung-surface implicated; but, as before remarked, not extraordinary in degree. The peculiar character of the dyspnœa of young children suffering from bronchitis is to be explained in great part by an attentive consideration of the altered size and relation of the air-cells here alluded to. Observe an infant who has for a few days been the subject of bronchitis. The upper part of the thorax hardly moves at all during inspiration; the lower part of the thorax below the nipple, at the same time, will be seen to be actually retracted. One part of the lung receives no air at all; another (more commonly the upper lobe, or a portion of it) receives air, but a less quantity than usual, owing to the emphysematous condition there existing.”

With respect to the diagnosis, Hewitt says:

“The movements of the chest are markedly influenced by the presence of emphysema. Emphysema, of what may be called *acute* form, developing itself in the lungs of infants or young children during an attack of bronchitis, most commonly attacks the upper lobes of the lungs. Here the want of expansion beneath the clavicles, the greater resonance on percussion, and the feeble respiratory murmur, are diagnostic of the presence of the lesion. Dr. Jenner states that he has observed a falling in or subsidence of the supra-clavicular region during inspiration, in cases where the apices are affected; and my own observations enable me to confirm this statement. Below the clavicles the emphysema may most surely be recognised; for in this situation the lung is most liable to be *extensively* affected. At other situations, the mixture of emphysema with apneumatoxis renders the diagnosis more difficult, the results offered by percussion are less valuable, and the auscultatory signs are less significative.

“An intensification of the natural respiratory murmur is not heard over parts of the lung which are truly emphysematous; rather coarse rhonchi may be heard masking more or less completely the natural respiratory sound; but these rhonchi themselves are not of much value as tests of the presence or absence of emphysema. The presence, at any situation, of signs indicative of apneumatoxis, such as dulness, finish rhonchus or complete absence of respiratory murmur, is almost or quite sufficient to warrant the conclusion that emphysema is also present, and in such cases the signs of emphysema have only to be looked for, to be at once made out.

“Inasmuch as emphysema is rarely unaccompanied by apneumatoxis, it is a matter of difficulty, as before remarked, to define precisely the share of each in the production of the various *symptoms* observed. The

dyspnœa present in bronchitis in young children is peculiar, the expiratory act occurring first, and the interval following the inspiratory act.

"When emphysema is present to a considerable degree, the 'expiratory' type of respiration is intensified; the respirations are eminently *shallow* in character, the cough is short, stifled and weak; and coincidently with these, the physical signs of emphysema at the apices, and of collapse at the lower portions of the lungs, may be satisfactorily made out. What of the symptoms and of the distress which the patient is labouring under, cannot be set down to the airless state of certain parts of the lungs, and to the obstructed condition of the air-tubes, must then be attributed to the emphysema, the emphysema affecting, let it be remembered, not limited portions of the lung's surface, but often spread over entire lobes. Orthopnœa and great distress are, in young children, generally due rather to the presence of extensive emphysema than to those other conditions which produce difficulty of respiration in bronchitis. As the child grows older, the lungs less readily become collapsed, and less readily, for this reason, does this acute form of emphysema occur. In weakly children the sternum often projects at the same time that the chest is laterally retracted, one diameter of the chest being increased to make up for the diminution of another.

"Of the symptoms of chronic emphysema in childhood there is little to be said, the characters of the disease differing in few essential particulars from those observed at a later period of life. As in the adult, the patient is liable to frequent attacks of bronchitis, evidently dependent on the existence of the emphysematous condition of the lungs. The rapidity with which severe dyspnœa supervenes on an apparently slight attack of bronchitis is, in ordinary cases, a good test of the presence of emphysema.

"Much more might here be said in reference to other symptoms which are produced in part or altogether, by the existence of emphysema, but the general indication thus given of the part emphysema ordinarily plays in giving rise to symptoms usually referred to other conditions, has been thought sufficient for the present purpose."

ART. 174.—*Some observations on the Dystrophia of Children.*
By Dr. R. KÜTTNER, of Dresden.

(*Journ. für Kinderkrankh.*, April, 1858; and *Dublin Quarterly Journal of Medicine*, August, 1856.)

In our daily intercourse, and, to some extent even in the field of science, the term atrophy has undoubtedly received far too wide an application and been confounded in its signification. Considering the phenomenon of far-advanced emaciation as the essential feature, a number of morbid conditions in little children have been only too often comprised under the name atrophy, which are marked by a deep depression of nutrition; and children have been looked upon as atrophic, who, on a more accurate examination, and stricter employment of the term, would be recognised as suffering from a general dyscrasia or an organic affection. The correction of this abuse and the restoration of

the word atrophy to its original more limited signification is, however, by no means so idle or unessential a matter as it may, perhaps, appear to many, but has, in addition to the greater accuracy in diagnosis to be thereby attained, also its special value in medical treatment.

The true atrophy, or—as we much more correctly denominate it—the dystrophia of children as an independent form of disease, *pædodystrophia*, *pædatrophia*, may, according to the signification of the word, necessarily comprise only that state of depressed nutrition and general exhaustion, which in little children is occasioned by the insufficient amount or the unsuitable nature of the food. This essential original dystrophy is, therefore, to be strictly distinguished from all those states of emaciation and exhaustion which, occurring as the product of morbid processes, bear a purely symptomatic, secondary character. While in the latter (*dystrophia symptomatica*) the eye of the physician must be constantly directed to the casual morbid condition, at one time a long-continued local affection, especially of the organs of digestion or respiration, at another a general dyscrasia, as tuberculosis or inherited syphilis; in the former (*dystrophia morbus*) the organization is originally, and, so far as morbid states do not accidentally concur, giving rise to complications, thoroughly normal, and suffers only from the want of the formative material so necessary to it. In the one case the physician must meet the affection by combating a morbid state; in the other, by administering suitable nourishment.

The development of genuine dystrophy is, as the organism constantly needs the access of nutritive matter, evidently possible at any time of life, but at no age is the tendency to it greater than in the earliest infancy. For, on the one hand, the delicate digestive organs of the child are by nature for a long time destined to one particular form of food, the mother's or the nurse's milk, all substitutes for which are only too apt to be insufficient; while on the other, the infantile system growing most rapidly, the more quickly and sensitively feels the want of suitable formative material. But dystrophy is exemplified not merely most frequently, but most accurately, in the young child, who, still for the most part free from products of disease, and still unaffected by exhausting bodily exertion or distracting mental affections, which, at a more advanced period of life, usually accompany a state of privation, and greatly modify its phenomena, only physically feels and exhibits the want of nourishment. For these reasons dystrophy has certainly been correctly admitted specially into the pathology of childhood, and considered as a particular affection of the earliest period of life, the time of suckling.

It is of practical importance to distinguish two species of dysentery according to their origin, whether the latter be attributable to a simple deficiency of nourishment (*dystrophia simplex*), or whether the nature of the food is at the same time unsuited and irritating to the delicate digestive organs of the child (*dystrophia dyspeptica*). The former, of course, occurs almost only in sucklings, starving on the ill-supplied breast of the mother or the nurse; the latter, by far the most common, is mostly the sad result of artificial feeding, which carries numberless children to the grave.*

* It is principally poor little children, committed to nurses, who are liable to this

A detailed description of the phenomena characteristic of the dystrophy of children might be superfluous to the readers of these pages: I shall, therefore, allude to only the most essential symptoms. Occupying the first place among them in a material point of view is the manifest deficiency of blood, exhibiting itself in the general paleness and in the diminished bodily heat. Hand in hand with it goes the insufficient metamorphosis of tissue, which, commencing with general relaxation and flaccidity, gradually leads to the complete disappearance of the cushion of fat, and finally gives the little sufferers an old or ape-like appearance. Among functional phenomena we have a constant craving for food, thrusting the little hands into the mouth; unceasing restlessness by day and night, which yields for only a short time to the administration of food, and at last passes into the highest degree of listless exhaustion; lastly, a very great diminution of the urine, often giving rise through the acidity of the concentrated secretion, to a kind of dysuria, and a frequently torpid solid and scanty alvine discharge, which is watery and diarrhœal only when catarrh of the bowel exists. This collection of symptoms presents the idea of dystrophy in its simplest form. In dystrophia dyspeptica we observe, of course, in addition, the phenomena of deranged digestive powers and of the collection of indigestible matters in the stomach and intestinal canal, by which these organs are brought almost into a state of chronic catarrhal irritation. Therefore, vomiting, the occasional formation of acid flatulence, and diarrhœa, erythema of the cavity of the mouth, with the development of fungi and of sores around the anus, appear as concomitant symptoms, which undergo numerous modifications in form and intensity according to the different nature of the deficiency in the nourishment. As secondary attendants on dystrophy, especially on its dyspeptic form, convulsive phenomena of all kinds may certainly occur, but they are in reality much rarer than is popularly supposed, the entire train of symptoms being only too readily attributed, not to its real cause, but to so-called inward spasms, and for these assistance is demanded. Public institutions, such as our Foundling Hospital, afford hundreds of opportunities of making observations of this kind. At one time the poor starved little ones are brought to seek relief for their constant uneasiness, their crying and their want of sleep; at another time, for their urinary affections; at another, for derangement of the bowels; but most frequently for the supposed inward spasms.

The post-mortem examinations in simple dystrophy present nothing characteristic in addition to the highest degree of deficiency of blood, as well as in general of insufficient nutrition of all the tissues. In dystrophia dyspeptica, on the contrary, we find traces of a catarrhal condition of the gastric and intestinal mucous membrane, with swelling and black punctation of Peyer's glands, or actual bursting and ulcerous destruction of the same. The fundus of the stomach, as well as some parts of the intestinal canal, are often found in a state of gelatinous softening, in consequence of the presence of contents which have passed into a state of acid fermentation, while

fate. A statistical table, deduced from the reports of the Foundling Hospital, shows, that while among the children under their mothers' care, the mortality among children under two years of age did not amount to quite 19 per cent., in those under the charge of nurses it attained to 29 per cent.; and was generally caused by dystrophy.

the liver appears at one time anæmic, at another overloaded with dark blood, but the bile is always remarkably thin and pale. That complicating morbid conditions, frequently hastening or directly causing death, which leave their traces in the dead body, as for example pneumonia and cerebral affections, must be distinguished from pure dystrophy, is self-evident. The swelling and hardening of the mesenteric glands in atrophic patients, on which so much stress was formerly laid (as Dr. Stiebel, jun., also has shown in his treatise 'On the Condition of the Mesenteric Glands in Childhood, and their relation to the atrophy of the first year of life' (Frankfort-on-the-Maine, 1854), are by no means constant, and at least in no case constitute an essential post-mortem appearance in dystrophy, often as the latter is considered and described as *tabes mesenterica*.

As I have already remarked in the beginning of this paper, dystrophy is among the most frequent causes of death in the first year of life, as a very considerable number of artificially fed children fall a sacrifice to it. Notwithstanding, the prognosis is not at all so unfavorable as one might at the first glance suppose, and in every case it is much better than in the symptomatic variety caused by diseases. It is truly surprising how quickly and completely such children often recover, and, indeed, even within a few weeks become so changed as not to be recognised, so soon as food adapted to their digestive organs and sufficient for their wants, has been supplied. If it is possible to transfer the suckling languishing on the empty breast of its mother, or the child wasting away on unsuitable artificial food, to a good nurse, care should be taken that if the latter appears unequal to her task, she should be exchanged for a better, the consequence of which will be the disappearance in a short time of all the symptoms arising from the insufficient supply of its wants, and the often rapid transformation of the little image of misery into the round, soft forms characteristic of infancy.

From what has been said it is evident that from the nature of the case, the treatment of dystrophy is not to be pharmaceutic, but that the entire task of the physician consists in the administration of nourishment quantitatively and qualitatively suited to the wants of the child. It is attributable only to imperfect knowledge of the affection, and of its causes, that the treatment is so often actually different. All those far-famed children's powders and syrups, with which nurses, and even physicians are so ready, to combat particular symptoms, are as unavailing as tonics, astringents, or narcotics. They are only signs of a false diagnosis, and easily contribute to aggravate the patient's state. Unfortunately, it is, however, often impossible to procure for the little sufferers the only efficacious remedy—sufficient and suitable nourishment—which mostly can be found only in a good nurse. This is particularly the case in public institutions, which, destined for the service of the poorer classes, are most frequently resorted to on the behalf of such sufferers, and yet can do nothing more than give suitable advice to the mothers or nurses. With the above view, we are in the habit of recommending, in the Dresden Foundling Hospital, for starving sucklings, the contemporaneous administration of good cow's milk, so as fully to satisfy the children, and, in those who are artificially fed, of regulating the diet as far as possible according to the principles which I have already pub-

lished in my aphorisms on the feeding of little children. Medicines I give only according to special indications, as in cases where complications exist. Instead of them, I have derived great benefit, especially in dystrophia dyspeptica, from administering, three or four times a day, from eight to ten drops of Malaga, Hungary, or port wine, to the little patients; and this plan I can under such circumstances the more strongly recommend, as, by prescribing the wine in the form of a medicine, the physician will no longer appear as an idle spectator, in the eyes of relatives, so often desirous of the administration of medicine. That this small quantity cannot act as a nutrient* is evident, but it acts as a condiment to the digestive organs, and makes them more capable of digesting and assimilating the food which is supplied often in sufficient quantity, but in a state of unsuitable preparation and admixture. Thus, at least, I believe the remarkable effect of wine under the circumstances above mentioned, which has been confirmed by hundreds of cases, is to be explained. When the dystrophy is removed, wine is of course again left off, as in well-fed, plethoric children it only too easily induces an insidious cerebral affection, even though given in small but long-continued doses.

ART. 175.—*Galvanism in Incontinence of Urine in Children.*

By Mr. SIMON, Surgeon to St. Thomas's Hospital.

(*Medical Times and Gazette*, Nov. 14, 1858.)

Mr. Simon, it appears, has treated successfully some cases of incontinence of urine in children by means of galvanism, the current being passed along a catheter which has been previously introduced into the bladder. The cases were those of incontinence from simple atony, and not those arising from irritable bladder.

* It is well known that Kletzinsky has discovered a considerable amount of lime in Malaga and Tokay wines, and that he ascribes to this fact a considerable share of their beneficial action.

REPORTS

ON THE

PROGRESS OF THE MEDICAL SCIENCES.

July—December, 1858.

THE intention of the following Reports is to pass in review the principal additions to each department of Medical Science, which have been placed on record during the preceding six months. It is not contemplated that they should be confined exclusively to the notice of what is new; any fact or doctrine which may be considered practically useful, will, although not strictly novel, be regarded as worthy of commemoration. It must be obvious to all who are aware of the immense mass of information which is almost daily put forth by the medical press of this and other countries, that the notice of every subject would be an impossibility. It therefore devolves upon the writers of each Report, to select only such articles for retrospection as may possess superior recommendations, either of an intrinsic character, or in relation to the main end and aim of all medical knowledge—the alleviation of suffering and disease.

I.

REPORT ON PRACTICAL MEDICINE.

The Institutes of Medicine. By MARTYN PAINE, .M.D, Professor of Institutes of Medicine and Materia Medica in the University of New York, &c. (Fourth Edition. New York : Harper Brothers, imp. 8vo, 1858, pp. 1095.)

DR. MARTYN PAINE is an anti-chemist, and anti-humoralist: he is also an utter unbeliever in the prevailing physical doctrines of life and disease. "Solidism and vitalism," he tells us, "will form the basis of these Institutes," and upon this basis, accordingly, he rears a vast superstructure of physiology, pathology, and therapeutics.

On this side of the Atlantic, we think that these doctrines will not meet with many ardent admirers; but Dr. Martyn Paine will meet with not a few who, in common justice, will study what he has written upon the reflex action of the nervous system, when they know what he lays claim to in this subject. Now what our author lays claim to here will better appear in his own words than in anything we can say, and we leave him, therefore, to speak for himself. For ourselves, we fear that the reader will lose himself not unfrequently in an interminable wilderness of words, and as frequently fail to find the idea which these words, according to their author, have been intended to express. We leave, then, our author to speak for himself, merely adding, that his statement is from an article in an appendix, headed 'Rights of Authors,' and dated New York, September, 1857.

"The author of these Institutes (and it will soon appear that he acted wisely) has sometimes thought it expedient to assert his claim of originality, in advance, to many doctrines promulgated in the work; as, for example, all that is most essential in the application of the nervous power, or reflex action of the nervous system to pathology and therapeutics, and to much of what is most important in the natural state of the functions. This may be readily seen by consulting p. 106, § 222 *b*, pp. 107—116, § 225—234, p. 295, § 476 *a*, p. 318, § 493 *d*, p. 321, § 496, 497, pp. 323—340, § 500—514,

p. 465—467, § 714—719, p. 506, § 803—804, p. 515—516, § 819 *b*, p. 661—663, § 894—896, p. 666—676, § 902 *b*—904, p. 679—680, § 905 *a*, p. 690—691, § 906 *g*, p. 693—695, § 917—923, p. 698, § 931—935, p. 703—711, § 940—952, p. 746, § 990½ *b*, &c., where all the subjects relate to the reflex action of the nervous system, and present the nervous power as an important vital agent in the various processes of organic and animal life, in the production of disease, in the operation of remedies, in all the results of bloodletting, in the changes which take place in the secreted and excreted products—having also originally set forth the agency of the nervous power in voluntary motion (index i, article *Will*), and as this power is the efficient cause of the modifications of organic results under the influence of mental emotion (index i, article *Mental Emotions*, and references at p. 867, § 1067). Indeed, as the reader will have seen, the foregoing doctrines relative to the reflex action of the nervous power, operating as a vital *stimulus*, or vital *depressant*, or vital *alterative*, as it may be modified in its nature by one cause or another (§ 107—109, 227—230, p. 661—662, § 894 *b*), pervade this work. The same doctrines are at the foundation of the author's 'Medical and Physiological Commentaries,' published in 1840, while the present work was published in 1847. In the mean time he has also laboured to inculcate them throughout his course of medical lectures in the University of New York—first on the 'Institutes of Medicine and Materia Medica,' from the year 1841 to 1850, and subsequently, to the present time (1857), on 'General Therapeutics and Materia Medica.'

"It may be worth saying, also, that the author preserves the term 'sympathy,' though always meaning by it, as he strictly defines, reflex action of the nervous system, and this whether he employs the term 'remote sympathy' or 'contiguous sympathy.' The elements of sympathy, as set forth in the work, are the nervous power and sensibility. All this will be readily seen by a reference to index i. Also, among other general remarks of a similar import, the author has the following :

"'Notwithstanding all the laws of sympathy that are necessary to the full interpretation of the remote effects of morbid and remedial agents are as well established as any laws in physics, they have not been applied to these important objects ; but, on the contrary, those philosophers who have contributed most to their critical exposition, overlook their pathological and therapeutical bearings, and cling to the doctrines of humoralism and of the operation of remedies by absorption ; nor have they applied, in the least, the nervous power in a philosophical manner to an explanation of the natural phenomena of sympathy' (p. 111, § 234 *a*).

"When the foregoing works were first published, it was in the midst of a universal prevalence of the chemical and physical doctrines of life and disease, and the author stood alone in the field of vital physiology, and in the application of the reflex action of the nervous system in resolving the great problems in physiology, pathology, and therapeutics. A few, however, had the quick sagacity to see its importance as presented by the author ; and since the decline

of organic chemistry began, others have entered upon the inquiry, and the most zealous have promulgated as original with themselves many of the doctrines which belong to the author of these institutes, especially such as are relative to the nervous system. But the author has relied upon his professional brethren for ultimate justice : ‘ *Ultimum et unicum remedium.*’ ‘ *Jus aliquando dormitur, moritur nunquam.*’

“ But the author has lately seen so great an indisposition, in certain quarters, to allow him any credit for his labours, that he has concluded to make this expostulation, which refers, particularly, to the following dispute about the authorship of matters in which neither of the gentlemen has any interest, but the writer alone of these Institutes. This rival claim appears in an article published by J. Adams Allen, A.M., M.D., in the ‘Medical Independent’ for September, 1857, p. 381, Detroit, Michigan. Thus :

“ It appears, from a late number of the London ‘Lancet,’ that M. Hall (Marshall Hall) recognises to a certain extent the priority of Dr. Campbell. His words are these :

“ ‘ I arrive at this conclusion : the idea and the designation of an excito-secretory action belongs to Dr. Campbell, and his details are limited to pathology and observation. The elaborate experimental demonstration of reflex excito-secretory action is the result of the experimental labours of M. Claude Bernard. My own claim is of a very different character, and I renounce every other. It consists in the vast *generalization* of excito-motory action throughout the system. I trust Dr. Campbell will be satisfied with my adjudication. There is the excito-secretory function as applied to pathology, an ample field of inquiry for his life’s career, and it is indisputably—his own. He first detected it, gave it its designation, and saw its vast importance.’

“ Dr. Allen then continues :

“ ‘ M. Hall thus far freely and fully admits the priority of Dr. Campbell, and the latter gentleman bases his claim upon the date, May, 1850. I shall undertake to show that this same doctrine was *first* publicly announced and illustrated in my lectures at the Indiana Medical College in November, 1848, and thenceforth continuously during the continuance of my public teaching before the several classes of that college, and also the medical classes of the University of Michigan, until my connexion with that institution “expired by limitation” in 1854. My own manuscript containing this doctrine was written in May or June, 1848’—that is to say, more than one year after the publication of these Institutes.

“ ‘ What I do claim is the great generalization that the excito-influence is followed by a reflex change in which the effect is not a motion, but a modification of vascular and nutrient action. That this effect takes place by means of the double nervous arc. A vast number of therapeutic phenomena are thus explained’ (as the reader will find, very extensively, in many chapters of this work, particularly in those upon remedial action, therapeutics, counter-irritation, cathartics, &c.)

“ Now, the whole of the foregoing doctrine is impressed upon the

'Medical and Physiological Commentaries,' and upon half of the pages of these Institutes, and has been always taught extensively in the author's lectures since 1841.

"Dr. Allen claims, also, the application of the principle to therapeutics, and remarks that 'in my course upon 'General Therapeutics' the subject of 'Counter-Irritation' came under review,' and concludes that 'the impression must be transmitted to the nervous centres, and thence reflected to the affected organ. In other words, the influence is primarily exerted upon the cerebro-spinal system, and secondarily upon the internal affected organ. This is the gist of the whole matter, and the point consists in the recognition of reflex cerebro-spinal action, which, in the instances adduced, give rise to a molecular or integral change in the inflamed tissue, and not a muscular contraction. The oral elaboration of this principle was suggested by an idea [?] which does not even now appear to have occurred to either M. Hall or Dr. Campbell, viz.: The motor effect is merely secondary, and not a necessary part of the action of the nervous arc.'

"Here, also, the whole of the foregoing doctrine appears throughout these Institutes. But they embrace a long chapter particularly upon 'Counter-Irritation,' in which it will be seen that the author has employed nearly the foregoing language of Dr. Allen, especially at pages 646, 647, § 893 *e*, and with a great elaboration and extensive application of the doctrine throughout the work; which had been also antecedently taught in his lectures for seven consecutive years before Dr. Allen promulgated the same views.

"To show still farther this partiality for the author's writings, or his lectures (then familiar to his large classes of students), he will quote from Dr. Allen the following conclusions.

" 'The effect is motory, if contractile fibre be present.

" 'The effect is secretory, if secretory organs be supplied.

" 'The effect is sensation, if sensitive neurine be reached.

" 'The effect is perception, or intellection, if the organ thereof be in connexion with the reflex nerve.'

" 'The effect produced, then, depends upon the structure and condition of the organ reached.'

" 'This influence is not confined to the mere increase of action, as the term excitor might perhaps suggest. The reverse might take place—the excitor may rather become the depressor. It would be as correct to say the depressor-motory, the depressor-secretory, as to say the excitor-idem.'

"Now the author of these Institutes not only dwells emphatically upon the depressing and sedative influence of reflex nervous action, according to the nature of the remote causes and special conditions of disease (p. 107—111, § 226—233³/₄, p. 507, § 806, p. 661—662, § 894—895, p. 671—672, § 904 *a*, p. 735, § 978, and references in § 1067 *a*, *b*, as to mental emotions, and in many other places), and upon its operation according to the natural structure and special vital constitution of organs, and their varying conditions (p. 59, § 129 *g*—*i*, p. 61—69, § 132—156, p. 73, § 163, p. 109, § 229 p. 111, § 233³/₄, p. 285, § 555 *d*—*f*, p. 313, § 487 *h*, p. 353—362, § 525—529,

p. 374—383, § 576—584, p. 415—417, § 649, p. 418, § 651 *b*, p. 421—423, § 657—658, p. 523, § 827 *c*, p. 542, § 854 *bb*, p. 613, § 892 $\frac{1}{2}$ *b*, p. 644—650, § 893 *c—i*, p. 665—672, § 902—903 *b*, p. 746, § 990 $\frac{1}{2}$ *b*, and the numerous references in those sections); but the author represents, also, the reflex action as variously alterative in organic life, and this imputed attribute pervades the author's writings. He enforces, everywhere, the doctrine that the reflex action of the nervous power is the modifying cause through which all the changes are effected by morbid and remedial agents in parts that are not immediately connected with the direct seat of their action; and, farther, that the principle is precisely the same when the nervous power is brought into operation by direct influences upon the nervous centres (as in the case of their diseases, or when the passions operate, or as the will determines voluntary motion), as it is when it is brought into operation in that indirect manner known as reflex action.

“Indeed, every one of the foregoing doctrines, in all their particularities, as quoted from the American claimant, are taught, at great extent, in the volume before us, as may be readily seen by consulting the references made in this protest, and, more extensively, index *i*, articles *Structure, Nervous Power, Sensation, Sensibility, Sympathy, Organic Functions, Remedial Action, Mind, Mental Emotions, Will*, —‘*Si quæris monumentum, circumspice.*’ It may appear superfluous, however, to have made these specific references in an article connected with the work itself; but it is done to encourage those readers who might not otherwise be inclined to ascertain the facts.

“But the writer is more interested with the European umpire, of whom he has felt that he has much more reason to complain.

“ ‘Omne animi vitium tanto conspectus in se
Crimen habet quanto major, qui peccat, habetur.’—JUVENAL.

“That the author's physiological and medical writings were generally known in Europe many years before the period at which ‘Dr. Campbell bases his claim’ (1850), is evident from the distinguished honours to which they had led in that country before that period—that from the Medical Society of Prussia as early as 1842—that from the Medical Society of Leipsic in 1843; and the ‘Medical and Physiological Commentaries’ (of 1840) were published simultaneously in London and New York, and as to the United States, the ‘Commentaries’ were early distributed throughout the land, and his ‘Institutes of Medicine’ more than a year, also, before Dr. A.'s lectures were delivered; and the author's lectures at the university, which form the groundwork of his Institutes, had been listened to annually by medical students from all quarters of the Union since the year 1841. In 1848 the author applied the doctrine of reflex nervous action to a physiological demonstration of the substantive existence of the soul and instinctive principle, which was then published in pamphlet form, and in 1849 the work was extended and assumed the shape of a book, and is now incorporated, in its essential parts, in these Institutes.

“Nor is that all; for the whole of this doctrine of reflex nervous action, and of the operation of the nervous power as an alterative, an excitant of the secretions and of vascular action (both direct and reflex), a depressant and sedative (according to the nature of exciting causes), and the great immediate cause of diseases and their cure—variously modifying organic actions—was set forth extensively and circumstantially in an ‘Essay on the Modus Operandi of Remedies’ in 1842, of which the author distributed, at that time, a large number of copies in London, and addressed four thousand copies to physicians throughout the United States. The author not only sent a copy of the work to Dr. Hall, but dedicated it to him (along with Prof. J. Müller and Dr. A. P. W. Philip) in connexion with an ‘Essay on the Philosophy of Vitality,’ and he may add that he controverted, in the former essay, doctrines of Dr. Hall (in ‘Memoir on Diseases and Derangements of the Nervous System,’ 1841), which were in direct opposition to those which are now in question (also, p. 296—297, § 476½, b). These essays were subsequently bound up in the third volume of the ‘Medical and Physiological Commentaries,’ where the former may be readily consulted. But Dr. Philip had fully deduced from his experiments the sedative as well as exciting influence of the nervous system upon vascular action before Dr. Hall’s experiments were made (§ 492).

“As to M. Bernard, his experiments bearing upon the connexion of the nerves with the function of secretion, however much they may have been varied and multiplied, were anticipated long before by those of A. P. W. Philip, which are quoted extensively in these Institutes (p. 290—321), and towards which Dr. Hall had no friendly disposition (p. 306—308, and where the writer has controverted his views). The merit of originality which belongs to the present writer, in relation to these experiments, consists in their extensive application in illustrating the functions of the nervous power as a vital agent, profoundly interested not only as an ‘excito-secretory’ power, and as a modifying cause of all secreted products, nutrition, &c., when diverted from their natural standard, but in deducing from them a universal agency of the reflex action of the nervous system, through ‘the double nervous arc,’ in the production and cure of disease, and by which he laboured to explode the chemical and physical doctrines as early as 1840. But that the writer may not be misapprehended, he will say that he endeavours to establish the fact that secretion in animals, as in plants, is conducted by powers implanted in every part, but that it is constantly influenced, physiologically, pathologically, and therapeutically, by reflex action of the nervous system.

“The writer is very sensible that unaccountable coincidences often present themselves in the development of new thoughts, and in the discovery of hidden things, especially where enduring reputation may be won. ‘*Ubi mel, ibi apes.*’—‘*Uno tiene la fama, y otro carda la lana.*’ But the reader, with these Institutes before him, will quickly find that much that is claimed by Dr. Hall, and all that he has granted to Dr. Campbell, in the foregoing quotation, and,

therefore, all that Dr. Allen appropriates to himself, abounds in this volume, and, in fact, constitutes the life and soul ('ζωή και ψυχή') of the work, as it does, also, of the 'Commentaries,' and of the essay on the 'Modus Operandi of Remedies;' nor can the reader fail of the conclusion that, were Dr. Hall's 'adjudication,' and Dr. Allen's after-thought, founded in any justice, and were not the claimants themselves the obnoxious parties, the present writer would have been long ago convicted by them and by others of arrogant assurance and the grossest plagiarisms. Nevertheless, the author is most happy to find that his solitary position is becoming relieved, and that a practical direction has been given to his labours by others which cannot fail of carrying forward the great doctrines at which he has toiled, and against manifold obstacles, during his professional life."

Researches on Gout. By Dr. GARROD, Professor of Materia Medica in University College, &c. (Proceedings of Royal Medical and Chirurgical Society, June 8, 1858.)

The author divides his communication into two parts: in the first are detailed the results of his analyses of the urine in the different forms of gout; the second is devoted to the consideration of the influence of the different forms of colchicum upon the urinary secretion. After describing the method of analysis employed in arriving at his results, and speaking of the opinions usually held upon the subject of the urine in gout, Dr. Garrod proceeds with the first part of his paper, the cases in which he subdivides into three classes.

1st Class.—Cases of acute gout, occurring in patients most of whom in the intervals of the attacks enjoyed pretty good health. About thirty analyses for uric acid made on the twenty-four hours' urine of several different patients are detailed, and a few for the determination of urea; and from these the following deductions are drawn. In the earlier stages of acute gout, the urine, for the most part, is small in quantity, and the uric acid, measured by the twenty-four hours' secretion, diminished; that this acid is thrown out in much larger quantities as the disease becomes mitigated, and that amounts much above the patient's daily average may be sometimes passed, forming critical discharges; and, lastly, the acid again becomes lessened, although not to the extent which occurs either just before or at the commencement of an attack. That the urea is usually thrown out in about the normal quantities, although its free elimination by the kidneys is often somewhat impeded, and, in consequence, the blood contains a small excess, which may possibly explain, in some measure, one peculiarity of gouty inflammation when it attacks superficial parts—namely, the presence of œdema and the subsequent desquamation of the cuticle. That occasionally a trace of albumen exists in the urine, but very seldom compared with its occurrence in the chronic forms of the affections.

The 2d Class included cases of chronic gout, the majority of which were not suffering from any urgent symptoms, but many were afflicted with some of the sequelæ of the affection, as shown by the concretions of urate of soda upon different parts of the body, and the stiffened condition of the joints. More than sixty analyses for uric acid are given in this class, made upon the day's urine of fourteen different individuals, and numerous determinations of urea are also detailed. The deductions from these observations may be thus summed up: The urine in chronic gout is usually rather pale in colour, below the average tint in the healthy subject, of low density, and increased in quantity. The amount of urea, except in extreme cases, the same as in health (due account being taken of the diet of the patient at the time the urine is passed). The uric acid is very much diminished indeed, and subject to excretion in very varied quantities at different times; and, lastly, the presence of a small amount of albumen is exceedingly frequent. Deposits in the urine are not of common occurrence in these subjects; but they occasionally occur on the cooling of the fluid, sometimes in the form of urate of soda or ammonia, at other times in crystallized and more or less coloured rhombs of uric acid.

3d Class.—Urine of individuals who had suffered more or less frequently from attacks of acute gout, of varying degrees of intensity, examined at the time of complete freedom from any symptoms of the disease. Several determinations are made on the urine of six individuals, and the following results arrived at. In no one of the six patients' urine did the amount of uric acid reach the quantity usually considered to be the average in health; in most of them it was far below, and it would appear that the kidneys in such individuals are apt to lose some of their excretory power for this body, a circumstance which must necessarily render the blood impure, and account for the liability which such patients possess to periodic visitations of this malady, and the great difficulty of effecting a radical cure of the disease.

Part 2 is devoted to an examination of the influence of colchicum upon the urine.—The usual opinions held by different authorities, as Christison, Chelius, &c., upon the *modus operandi* of this drug are first related, and many of the analyses brought forward in their support are shown to be very fallacious, the error arising not from any fault in the analyses themselves, but from small specimens of urine, passed at particular times of the day, being examined, and no account taken of the twenty-four hours' elimination. Between fifty and sixty analyses, showing the amount of the uric acid eliminated, are given, together with numerous determinations of the urea. The results of these observations on the action of colchicum were as follows:

In one case, where no gouty affection existed and no febrile disturbance was present, colchicum appeared to have the effect of slightly diminishing the quantity of urine, and likewise of diminishing somewhat the excretion of uric acid.

In a second case, similar to the above, the influence of the medicine was notably to diminish the quantity of urine, the uric acid being

slightly increased, but the increase was less than a quarter of a grain per diem.

In case 3, a gouty patient recovering from an acute attack, the uric acid was somewhat increased during the administration of colchicum, but not in a greater degree than frequently occurs under such circumstances when no medicine is given.

In case 4, both the urine and the uric acid were diminished by the influence of the drug; and so on for the other cases.

The author considers, as the results of his analyses—

1st. That there is no evidence to prove that colchicum produces its effects upon the system by causing the kidneys to excrete an increased amount of uric acid, but that, in fact, the reverse would seem to hold good.

2d. That colchicum is not always a diuretic, but often diminishes the renal secretion, especially when its action is exerted upon the alimentary canal.

3d. That colchicum has no marked influence on the excretion of urea.

An appendix to the paper contains the results of seventy-two analyses, performed on consecutive days, of the urine of a gouty patient; and the results exemplify in a marked manner the peculiar mode in which uric acid is frequently eliminated in such cases, and also tends to confirm the conclusions arrived at with respect to the action of colchicum upon the excretion of this principle.

An Essay on Wasting Palsy (Cruveilhier's Atrophy). By WILLIAM ROBERTS, B.A., M.D. Lond., Physician to the Manchester Royal Infirmary. With four lithographed plates. (8vo, London, Churchill, 1858, pp. 210.)

The purpose of this volume is to collect together what is now known respecting a peculiar species of palsy, of which the most striking character is a remarkable wasting of the disabled muscles which is independent of lead poisoning. On the Continent, since 1850, the subject has attracted considerable attention, and Cruveilhier, Aran, Duchenne, Oppenheime, Wachsmuth, and others, have made it the object of special study; but in this country the existence of the disease has been scarcely recognised, and the characters very imperfectly known, although, as Dr. Roberts clearly shows, in his historical introduction, cases were noticed and fully described, even before it had attracted attention elsewhere, by English writers of a former day—Cooke, Caleb H. Parry, Charles Bell, Abercrombie, Darwell, and Herbert Mayo. The name of *wasting palsy*, which is adopted by the author, will, in all probability, be a favorite with English ears—a greater favorite than another name which he also suggests, namely, *Cruveilhier's paralysis*, or *Cruveilhier's atrophy*. At any rate, we are glad to find any fair English equivalent for some of the roundabout modes of expressing the same thing, as the *atrophie musculaire progressive* of Aran, the *paralysie musculaire atrophique* of Cruveilhier, the *atrophie musculaire avec transformation grasseuse* of Duchenne, and so on.

Dr. Roberts has spared no pains in the execution of his task, and the manner in which he has succeeded is deserving of very high praise. In order to obtain a correct portrait for diagnosis, he gives in detail seven cases—five falling under his own notice, one by Cruveilhier, and the seventh by Dr. Th. Valentiner; and after this he tabulates with great care the salient particulars of no less than 105 cases. He furnishes us, indeed, with all the information which is to be met with, or which patient research can elicit; and if the disease be not familiar to us now, it must be simply from inattention to the picture which is here drawn for us. In the table the cases are arranged in two divisions (an arrangement first proposed by M. Aran), according as they are *general or partial*. After the cases the author proceeds to consider a number of general questions naturally arising out of their anatomical distribution and symptomatology, invasion, course, determination, etiology, post-mortem appearances, nature, diagnosis, prognosis, and treatment.

The following table gives a comprehensive view of the anatomical distribution of the atrophy in 60 cases of the partial type. It requires no comment.

	Total No. of times affected.	No. of times exclusively affected.	Both sides together.	Right side only.	Left side only.
Upper extremities . .	55	48	31	17	7
Hands	39	7	17	17	5
Forearms	30	0	15	11	4
Upper arms	32	0	18	8	6
Shoulders	33	3	22	6	5
Lower extremities . .	12	6	8	1	3

The symptoms are considered under the separate heads of lesions of motility and lesions of sensibility. The chief of the lesions of motility is loss of power; the secondary ones are fibrillary tremors, cramps, twitches, and diminution of electric contractility. In uncomplicated cases there are no lesions of sensibility. Tactile sensibility retains its delicacy in the skin over the affected muscles. When the tongue is attacked, neither the gustatory sense nor its common sensibility is in the least blunted; and, according to Duchenne, the special muscular sense survives to the last. There is neither pain, numbness, nor any other abnormal feeling. But matters do not always pass thus, and in a large minority of cases the sensory nerves are untowardly affected; and pain, sensitiveness to cold, and even slight numbness, are added to the more constant

symptoms. Neuralgia, either in the nerves of the diseased part or elsewhere, is also said to be a not uncommon *complication*. The general and mental health are not affected.

The invasion of wasting palsy is usually slow and insidious ; the starting point generally in the upper limb, and in the hand particularly ; the course is measured, not by days and weeks, but by months and years ; the termination may be in recovery, permanent arrest, or death. With reference to prognosis, Dr. Roberts says, " the longer wasting palsy has continued, the less amenable it is to treatment, and the more remote is the prospect of recovery. No case which has lasted above two and a half years has hitherto issued in recovery. With regard to recovery from a partial atrophy, which has become stationary, the same remarks hold good. After a year or two, if no amendment has set in, I fear the foregoing histories do not warrant any hope of recovery."

Cold and overwork stand out as among the most important causes, and on this head Dr. Roberts says, " it may be mentioned that when cold is the source of the atrophy, it is much more apt to extend to the muscles of the trunk than when it arises from overwork. Of the 25 cases attributed to overwork 18 were partial, and only 7 general ; whereas, of the 16 charged to the agency of cold, 6 were local and 10 general. It is a much more serious matter, therefore, for the atrophy to be tracked back to cold, or a wetting, than to overfatigue of the muscles. As might have been expected, in more than one instance the disease is alleged to have arisen from cold combined with fatigue."

The varying and conflicting results of post-mortem examination are summed up as follows :

" The nervous system was examined in 13 cases.

" In two of them the spinal centre was sound, but the anterior roots and peripheral distribution of the muscular nerves were extensively diseased.

" In three cases, there was inflammatory softening of the cord ; accompanied, in two of them, with fatty degeneration and destruction of the anterior roots, and in one of these of the peripheral muscular branches also. In the third case, the anterior roots and peripheral branches were healthy.

" In one case, there was amyloid degeneration of the cord, confined to the posterior median columns, and a granular degeneration of the peripheral muscular branches, but with entire preservation of the anterior roots.

" In two, the nervous centres and anterior roots, were found, on the most rigorous investigation, perfectly sound.

" In the remaining four cases, the nervous system was pronounced healthy so far as the examination was conducted.

" I have tabulated these results as follows :

Nos. of the cases in the table of abstracts.	Spinal cord.	Anterior roots.	Peripheral distribution.
71, 72, 87	Healthy.	No mention.	No mention.
73, 74	Healthy.	Atrophied.	Atrophied.
83	Healthy.	No mention.	Healthy.
79, 89, 90	Healthy.	Healthy.	No mention.
94	Amyloid degeneration.	Healthy.	Atrophied.
95	Partly softened, partly indurated.	Healthy.	Healthy.
97	Softened.	Atrophied.	Atrophied.
98	Softened.	Atrophied.	No mention.

The post-mortem examinations, the clinical facts, and the deductions of physiology, in Dr. Roberts's opinion, all go to show that no lesion, acting downwards from the nervous system to the muscles, can produce the phenomena of wasting palsy; and with Duchenne, Aran, Oppenheimer, Wachsmuth, and Dr. Meryon, we have to look for the primordial phenomena of the affection in the muscles themselves—probably in a fatty and granular degeneration of the fibre. It is fully admitted, however, that the nervous and muscular systems will sympathise with and react upon each other in this diseased state. We may look, our author says, for three orders of phenomena:

“1. *Primary or direct*.—These are, destruction of muscle and consequent loss of power. They are necessarily always present, and, in the simple cases, are the sole factors in the problem.

“2. *Secondary or reversed*.—These include atrophy and fatty degeneration of the motor nerves, together with softening or other change in the spinal cord. These are not invariably present. Probably some of the neuralgic pains should find a place here; the nervous filaments ministering to the muscular sense, being involved in the general devastation, may be the source of those severe pains sometimes experienced in the substance of the affected muscles; but I am disposed to believe that most of the abnormal sensations and movements are the expression of the next order.

“3. *Tertiary or reflected*.—These may be produced through irradiation of impressions conveyed to the spinal centre by the nerves of the special muscular sense; or they may be the direct consequences of the secondary organic changes just described as going on in the part of the cord where the nerves of the decaying muscles originate, whereby the contiguous sound portions are irritated. In both these ways abnormal movements and feelings are provoked—undue sensitiveness to cold, neuralgic pains along the nervous trunks or in the joints, twitches, cramps, contractions, and fibrillary tremors.”

The treatment advocated divides itself into two lines of indication.

“The object of the first is to arrest or cure the atrophy of the

muscles, and that of the second to combat the secondary phenomena. In dealing with the primary disease, our first endeavour must be, to seek out the *exciting cause*, and obviate, if that be practicable, its continued operation. The handicraftsman must immediately renounce the practice of his mechanical art; the labourer must take off the strain from the overworked members. If cold and damp be the original excitant, they must be sedulously avoided in future, to escape the risk of a recrudescence or relapse. The *direct* treatment must have for its end, in the active stage of the malady, to restore the nutritive operations from their depraved estate to their original healthy tenour. This is accomplished, by a judicious combination of perfect repose and regulated stimulation. Experience has shown that, for the latter purpose, no remedy approaches galvanism, which should be applied to the muscles daily, or every other day, in the manner above directed. With galvanism may be combined gentle frictions of the parts affected, with some stimulating liniment; and, if the means be at hand, warm sulphuretted or saline baths may be employed occasionally, as adjuvants to the local applications. When the disease has become stationary, more violent stimulation by electricity and friction must be resorted to, and cold bathing, with enforced exercise of the muscles, may be tried, if other means fail of effect. Remedies should be applied without loss of time, for when the atrophy has extensively affected the muscles of the trunk, or has remained stationary for a lengthened period, the chance of recovery diminishes in an accelerated ratio.

“The *secondary* phenomena, neuralgic pains, spasms, &c., are most effectually subdued by warm baths and inunctions; or, if they obstinately persist, by morphia-dressed blisters over the painful tracks. Any ailment of the general system, or concurrent and independent disease, must be dealt with, in accordance with the recognised canons of therapeutics.”

On Cerebellar Hæmorrhage. By Dr. J. R. HILLAIRET, Physician to the Hospital for Incurables in Paris. (‘Archiv. Gén. de Méd.’ February, March, April, May, 1858.)

This very elaborate and valuable memoir is divided into two parts; one part containing eight cases, six of them original; the other part containing a general description of the two principal forms of the malady, an examination of each of the more important symptoms, and a comparison between cerebellar and cerebral hæmorrhage.

Cerebellar hæmorrhage, according to M. Hillairet, occurs under two forms: one slow, and progressing regularly from the attack until death; the other sudden, and almost immediately fatal. The last is the most frequent of the two.

In the first form the consciousness is preserved at the moment of the attack; in the second form the consciousness may be lost at the moment of the attack, but only momentarily.

In both forms, but particularly in the first, the attack is followed by vomitings, which recur frequently during the course of the malady,

and will not be stayed. M. Hillairet considers that vomiting is a symptom which has some special relation to cerebellar hæmorrhage, or to some lesion which may bring about an augmentation in the volume of the cerebellum, and he differs with those who think that it is a symptom of cerebral hæmorrhage. In some cases the vomiting has coincided with laceration of the gray substance of the periphery, but in all cases the author refers this phenomenon to some slight compression or irritation of the pneumogastric nerves, which compression or irritation is brought about by the pressure of the blood effused within or upon the cerebellum. Vomiting is also observed in other affections of the cerebellum, which are attended by augmentation in the volume of this organ, such as abscess, cysts, tubercles, and, but very rarely, in cases of softening where the volume of the organ is not sensibly increased, and where, consequently, the pneumogastrics are not subjected to the irritation of pressure. In a word, M. Hillairet differs from the majority of writers on this subject, and considers that vomiting is of very rare occurrence in cerebral hæmorrhage, unless the cerebellum is implicated. Thus, according to his experience, there was vomiting only in 1 case in 30 where the hæmorrhage was cerebral, but as much as 1 in 2·30 where it was cerebellar.

In sudden and severe cerebral apoplexy the patient falls without sense and consciousness, breathes stertorously, and, as a rule, death does not happen until after an interval of some hours; in sudden and severe cerebellar apoplexy, on the contrary, the patient is often able to give expression to his suffering at the moment of attack by a cry, a gesture, or in actual words, and except for a short time before death, which happens speedily, the intelligence is not completely abolished.

Hemiplegia is not so frequent a symptom of cerebellar apoplexy as has been supposed. It has been said to be present in a third of the cases; in the cases recorded in the present memoir it was not met with. The paralysis has been always on the opposite side of the body; at the same time it is possible, as M. Longet has shown, that in some cases a want of decussation may lead to paralysis on the same side. In no instance would there appear to be conclusive proof that the paralysis was either general or confined to the lower part of the body. In many cases the paralysis is incomplete, and the limbs relaxed rather than palsied. Thus, the patient will be unable to preserve his equilibrium if he attempts to stand; but if he lies down, he is able to move his legs about, or even to raise them from the bed and maintain them in an elevated position.

There was no tendency to move backwards or to turn round and round in any of the cases contained in M. Hillairet's memoir; but in one of them the patient, before the attack, complained of being pushed by some irresistible force towards the left side.

Paralysis on the opposite side of the face, which is habitual in cerebral hæmorrhage, is only met with in exceptional instances in cases of cerebellar hæmorrhage. In the latter cases, also, deviation of the tongue is quite exceptional; but notwithstanding this, the speech, as a rule, is dull and drawling. Only once in 26 cases was

there any deviation of the commissures of the lips. M. Hillairet also directs attention to a silly astonished expression of countenance, accompanied by a fixed stare, as being present at the time of the attack; but this phenomenon may easily be overlooked, for it soon passes off.

In severe cerebellar apoplexy the sensibility would at first seem to remain uninjured. Once, about the middle of the course of the disorder, M. Hillairet observed a momentary state of hyperæsthesia. Once only was the sensibility lost from the beginning. Afterwards, as a matter of course, the sensibility disappears as the state of coma gains ground.

The special senses are affected the same way as common sensibility. At the beginning it is quite exceptional for the sense of hearing, smelling, or tasting, to be at all impaired. Sometimes, when the *processus cerebelli ad testes* are injured or destroyed, sight may be enfeebled or abolished. It would seem also that contraction and immobility of the pupils are the rule, dilatation the exception.

Convulsions are not met with in cerebellar hæmorrhage, if the mischief be confined to the organ, or in cases where the effused blood has set up an inflammatory action in the surrounding cerebellar structures.

Most generally the bowels do not act, but sometimes there are involuntary evacuations. In the form of apoplexy which is slow and progressive, the power of passing water is preserved at first; in the sudden form it is lost from the beginning.

The mean duration of cerebellar apoplexy, not including "les cas foudroyants," is a day and a half.

In a word, there are certain common symptoms, which belong equally to cerebral and cerebellar apoplexy, such as general headache, vertigo, dimness of sight, ringing in the ears, hemiplegia, and so on; but there are also certain differences which may help us to a diagnosis, and M. Hillairet's memoir is an important contribution to the knowledge of these differences.

The physiology and pathology of the Central Nervous System. By E. BROWN-SÉQUARD, M.D. ('Lancet,' various Nos., from July 3d to December 18th, 1858.)

The following lectures, which contain the most recent statement of the views of this distinguished physiologist upon this subject, were delivered before the Royal College of Surgeons of England, in May, 1858.

Lecture I.—This is devoted chiefly to the setting forth of the truth of Sir Charles Bell's theory as regards the existence of two distinct sets of nervous conductors—the sensitive and the motor. With respect to the pain produced by the irritation of the anterior roots of nerves (the "recurring sensibility" of Majendie)—a phenomenon which has been urged as an objection to the purely motor character of these roots—the explanation given by Dr. Brown-Séquard is, that the irritation gives rise to cramp, which cramp is the cause of the pain, and his conclusion is, that there is no sensibility of any kind in the anterior roots, and that it is

because they are motor, and not because they are sensitive, that they cause pain when they are irritated.

Lecture II.—In this lecture Dr. Brown-Séguard relates certain experiments which show that the transmission of the sensitive impressions along the spinal cord is chiefly in the gray matter. After refuting the views of Longet, and exposing some principal causes of error in experimenting, he relates certain experiments to show that the part, though not itself endowed with sensibility, may be a conductor of sensitive impressions. After this he relates experiments which prove, 1st, that a transversal section of the posterior columns of the spinal cord, instead of causing anæsthesia, is followed by hyperæsthesia; and, 2dly, that a transversal section of the whole spinal cord, except the posterior columns, is followed by a complete anæsthesia. As a general conclusion, it would seem that the conductors of sensitive impressions, at their arrival in the spinal cord, either enter directly into the central gray matter, or go up or down a little way in the posterior columns also, most likely in the posterior gray cornua, and in the posterior part of the lateral columns, and entering afterwards the central gray matter, by, or in which, the two sets ascend towards the encephalon. It would seem also, that the posterior columns of the spinal cord are not, as has been imagined, a bundle of fibres, from the posterior roots of the spinal nerves; and that the restiform bodies, which are the continuations of the posterior columns, are not a place of passage of any of the conductors of the sensitive impressions of either the various parts of the trunk and limbs, or of the head; and that, therefore, the cerebellum, with which the restiform bodies are connected, does not receive from them any of its conductors.

Lecture III.—In this lecture, Dr. Brown-Séguard shows that the celebrated experiments of Galen, which have been always thought to prove that there is no decussation of the conductors of sensitive impressions in the spinal cord, do not prove anything in this respect; and after this he proves, we think conclusively, that *the conductors of sensitive impressions make their decussation in the neighbourhood of the place of insertion of the sensitive nerves, or roots of nerves, in the cerebro-spinal axis.* As regards the sensitive fibres of the trunk and limbs, the following experiments show that their *decussation takes place in the spinal cord.*

“1st. In a mammal the spinal cord is laid bare at the level of the two or three last dorsal vertebræ, and a lateral half of this organ (including the posterior, the lateral, and the anterior columns, and all the gray matter, on one side) is divided transversely. The animal is left at rest for a little while, and then it is ascertained that sensibility seems to be much increased in the posterior limb on the side of the section, while it seems to be lost, or extremely diminished, in the posterior limb on the opposite side. There seems to be, therefore, *hyperæsthesia* behind and *on the side* of a transversal section of a complete lateral half of the spinal cord; while, on the contrary, there seems to be *anæsthesia* behind the section, and *on the opposite side.*

“This experiment is one of the two made by Galen; but he seems not to have looked at all at the condition of sensibility, and he simply states that there is a paralysis on the side of the section, and no paralysis on the opposite side.

"Schœps, Van Deen, and Stilling have observed that sensibility is not lost in the limb or limbs behind and on the side of the section of a lateral half of the spinal cord; but they have not remarked the most important fact, that on the opposite side there is anæsthesia. They also do not mention this curious result of this experiment, the existence of hyperæsthesia on the side of the injury.

"2d. If, after having made a first section of a lateral half of the spinal cord in the dorsal region, on the right side, for instance, and after having ascertained that the *right* posterior limb is hyperæsthetic, or at least extremely sensitive, we divide the *left* lateral half of the spinal cord in the cervical region, we find then that the *right* posterior limb loses entirely, or almost entirely, its sensibility. This experiment shows clearly that the sensitive impressions coming from the *right* posterior limb, after the first section, passed across the spinal cord from the right into the left side, along which they were transmitted to the encephalon.

"3d. To obtain a very striking result from the experiment, which consists in only one section of a lateral half of the spinal cord, it is better to make it after the posterior columns have been divided. We know that after this division there is hyperæsthesia in the parts of the body which are behind the section; if, after having ascertained this fact, the section of a lateral half is completed where the posterior columns have been divided, we find that the hyperæsthesia seems to increase in the side of the second operation, while, in the opposite side, not only the hyperæsthesia, but sensibility entirely disappears.

"4th.—There is another mode of proving that the conductors of the sensitive impressions decussate in the spinal cord. In several points of view this mode of proving is superior to the preceding. It consists in a longitudinal section of the spinal cord, an experiment already made by Galen, but the results of which, as regards sensibility, have been overlooked by him.

"The spinal cord is laid bare in the whole lumbar region, and a careful division of the entire extent of the part of the organ giving origin to the nerves of the posterior limbs, is made so as to separate the two lateral halves of the organ, one from the other. If this experiment could be executed perfectly well, nothing would be divided in the cord except the commissures, which unite the right side with the left side of the cord, and all the longitudinal elements of this nervous centre would be left uninjured; but it is impossible not to cut more or less on either side. However, when the operation has succeeded well, *i. e.*, when the two separated halves have been very little injured, a striking result is obtained. The voluntary movements still exist in the posterior limbs (though diminished on account of the injury to the muscles of the lumbar region), but *sensibility is entirely lost* in them. For persons who know that injuries to the spinal cord, which cause a diminution of sensibility, always produce a greater diminution of voluntary movements, this fact will not be explained by the supposition that some injury has, then, been made to the two halves of the cord, and that it is, in consequence of this supposed injury, that the loss of sensibility is due. At least it will, I think, be easily admitted that if the two lateral halves of the cord had been injured enough to produce a complete and a lasting anæsthesia, there would be a notable degree of paralysis of voluntary movements.

We repeat that such is not the case: the animal has the use of his two limbs; he moves about pretty freely, as Galen had already said. The loss of sensibility, therefore, must depend on the section of the commissures of the spinal cord, or, in other words, on elements of this organ which cross each other in the median line, or, rather, in the median plane.

"If now we compare the results of this experiment with those of a transversal section of a lateral half of the spinal cord, we find that they agree perfectly in showing that the conductors of the sensitive impressions decussate in this organ.

"5th. Another experiment, which is a combination of two of the preceding, gives a still better proof of the decussation of the conductors of sensitive impressions in the spinal cord. A longitudinal section is made on the cervico-brachial enlargement of the spinal cord, so as to separate it in two lateral halves. I ascertain then that sensibility is lost in the two anterior limbs, while it remains, and even seems to be increased, in the two posterior limbs. Of course, if the loss of sensibility in the two anterior limbs depended upon an injury to the two sides of the cord, and not upon a section of the decussating conductors of sensitive impressions, there would be a loss of sensibility, or, at least, a diminution of it in the posterior limbs. The admission of a decussation explains the two facts—loss of sensibility in one set of limbs, and conservation of it in the other set. If we divide transversely, in the same animal, the right lateral half of the spinal cord, we find then that the posterior limb on the same side becomes more evidently hyperæsthetic than before, and that the left posterior limb loses its sensibility. The transmission for this last limb, therefore, took place by the right half of the cord, while that for the right posterior limb continues to take place by the left half of the cord."

As far as experiments go, it is very difficult to decide whether the decussation of the conductors of sensitive impressions is absolutely complete or not, but it seems to be very nearly, if not absolutely, complete. In reptiles and birds the decussation is not so complete and immediate as in mammals; in man (judging from certain pathological cases which will be considered afterwards) it would seem to be complete.

Lecture IV.—One question discussed in this lecture is—how is it that sensibility is not lost, but only more or less diminished, although the spinal cord is deeply altered? This question seems to have its solution in the following experiment:

If we divide transversely the posterior columns in the upper part of the lumbar region in a mammal, we find that there is hyperæsthesia *everywhere* behind the section; if, then, we divide the posterior parts of the lateral columns and the posterior gray horns, we find that the hyperæsthesia increases also *everywhere* behind the section. If the section is carried further, so as to have the whole posterior half divided transversely, the posterior part of the gray matter, behind the central canal, being cut, the hyperæsthesia remains excessive *everywhere* behind the section. When another section is made, cutting a little more of the central gray matter, the hyperæsthesia disappears from *everywhere* at once, and a certain degree of anæsthesia appears also *everywhere* behind the section. At last, if the whole of the central gray matter be divided, with also a good part of the basis of the anterior horns, sensibility is very much diminished

everywhere behind the division, and it disappears entirely *everywhere* at the same time when the section has left only the anterior parts of the anterior columns. The general result of this experiment is, that any change that takes place in the state of sensibility,—either an increase or a diminution,—shows itself *everywhere*, at the same time, behind the section.

Other questions discussed have reference to the transmission of the various sensitive impressions—touch, pain, temperature, muscular contraction, &c.—and of the orders of the will to muscles. Dr. Brown-Séquard thinks that each kind of sensitive impression requires a distinct conductor. With respect to voluntary movement, he is of opinion that the idea of two columns of the spinal cord (the anterior) alone employed in the production of these movements, must be abandoned, and that it is extremely probable that the true conductors pass in the anterior pyramids, and, after having made their decussation, pass chiefly in the lateral columns of the spinal cord and in the gray matter near these columns, and, at last, after a short distance, a number of these conductors leave the lateral columns to pass into the gray matter and into the anterior columns.

Lecture V.—This lecture is occupied chiefly with pathological cases which show that the transmission of sensitive impressions is chiefly through the gray matter of the cord, and not along the posterior columns. One case is this :

CASE 14.—J. C—, æt. 44, admitted into St. Bartholomew's Hospital for paraplegia. The patient was lifted into a chair, and when thus sitting, he did succeed, by a great effort, in raising his legs from the ground ; but afterwards the inability of motion became complete through each lower limb in its entire extent. There was no discoverable impairment of sensation in any part of the limbs ; on *scratching, pricking, and pinching the skin* nowhere was any defect of feeling acknowledged by the patient. In the upper limbs there existed no defect either of motion or sensation.

Autopsy.—The spinal cord was the only seat of disease ; membranes healthy. The posterior half, or columns of the cord, throughout the entire length, from the pons to the other end, was of a dark-brown colour, extremely soft and tenacious. The anterior half exhibited its natural whiteness and firm consistence. The roots of the spinal nerves were unaltered ; the brain was healthy. (Mr. Edward Stanley, in 'Medico-Chirurgical Transactions,' 1840, vol. xxiii, pp. 83-83.)

Lecture VI.—In this lecture, which is a continuation of the former, we have also some very important cases. Of these this is one :

CASE 22.—A man, æt. 44, after having had cramps, formication, and weakness in the lower limbs, and paralysis of the upper limbs, for a long period, was admitted at the Charité. Sensibility existed everywhere. On the evening of the 1st of November he was able to walk with aid of some one. Sensibility remained everywhere to the last moment before his death, on the 3d of November, at three a.m.

Autopsy.—Encephalon normal. There is an induration of the spinal cord from its upper extremity to the third dorsal vertebra, and from the sixth dorsal vertebra to the lower extremity. The tissue of the cord in these parts being cut, was shining, looking like porcelain, hard and difficult to be crushed. The gray matter was also a little harder than normal, but of its usual colour.

The anterior and posterior roots seemed normal. In the space between the third and sixth dorsal vertebræ the cord was softened, pultaceous, resembling a whitish or rather slightly rose pulp (*bouillie*), punctuated in some places. Put in water, many parts became disintegrated, and formed a kind of emulsion. This alteration existed only in the white substance; the gray, on the contrary, seemed to have preserved its normal consistence. The microscope showed that the gray matter in both the softened and the indurated parts contained normal cells and fibres, and normal blood-vessels, while the white substance in the softened region contained but rare fibres, which were altered, containing an oily matter and granulations. There was also a quantity of granulated corpuscles of inflammation, with many capillaries, oily drops, and amorphous matter. In the indurated white substance there was less alteration, and the fibres were more normal and numerous. (Laboulbène, in 'Mémoires de la Société de Biologie,' for 1855, pp. 233-45.)

The author of the report of this case adds, that he has ascertained that sensibility to pinching, pricking, touching, and tickling, and the feeling of cold, and that given by a muscular spasm due to galvanism, have persisted in this patient, although the white matter, *i. e.*, the posterior and the antero-lateral columns, had but few, and only altered fibres. A capital point in this case was the conservation of the gray matter with the persistence of sensibility.

Summing up the pathological evidence it is found, 1st, that there are many cases of alteration only, or almost only, limited to the gray matter, in which both voluntary movement and sensibility are lost; and, 2dly, that there are many cases of deep alteration of all the white substance of the spinal cord, the gray matter remaining normal, in which sensibility persisted.

Lecture VII.—In this lecture Dr. Brown-Séquard relates cases which show that the conductors of sensitive impressions from the trunk and limbs decussate in the spinal cord, and not in the encephalon, and that the conductors of the orders of the will to the muscles decussate in the lower part of the medulla oblongata and not in the pons varolii.

Of cases proving the existence of a decussation of the conductors of sensitive impressions in the spinal cord, these are very remarkable :

CASE 29.—A patient was admitted into the St. André Hospital. He had paralysis of voluntary movements in the *right* side of the body, in which sensibility was preserved. In the *left* side, on the contrary, the voluntary movements existed, but there was a great diminution of sensibility.

Autopsy.—There was a fungoid growth (*végétation fungoïde*) pressing upon the *right* lateral half of the spinal cord. ('Mémoires de la Société de Biologie' for 1854.)

CASE 30.—A patient had lost voluntary movements in the two limbs of the *left* side, in which sensibility was preserved. In the right side sensibility was much diminished (*très obtuse*).

Autopsy.—A clot of blood was found in the *left* lateral half of the spinal cord in the cervical region. ('Mémoires de la Société de Biologie' for 1854.)

These two cases are extremely valuable, and they agree perfectly with the results of experiments on animals. It is so also with the following cases :

CASE 31.—A man, after having felt a sudden pain in his back, became incompletely paralysed of voluntary movements in the *right* lower limb.

Sensibility was not altered in this limb, but in the *left* side, where voluntary movements were not impaired, sensibility was entirely lost from the breast to the foot.

Autopsy.—Brain and its membranes normal. In the spinal cord an hæmorrhage had taken place, and blood was found in the *right* side of the gray matter, having destroyed also its horns, and a part of the left anterior column in the dorsal region. (Monod, in 'Bulletin de la Société Anatomique,' No. XVIII, p. 349, Obs. 3, and in Ollivier, *loc. cit.*, vol. ii, p. 177.)

This is a very remarkable case, teaching, not only that there is a decussation of the conductors of sensitive impressions in the spinal cord, but also that the gray matter is the principal channel for these impressions.

In the three preceding cases there is no mention of hyperæsthesia, although it must have existed on the side of the injury in the spinal cord; we shall find it mentioned in the following cases, and especially in the next one, which we give almost in full, on account of its extreme importance :

CASE 32.—On the 4th of February, 1850, a man, æt. 28, was admitted into the St. Louis Hospital, in Professor Nélaton's ward, a short time after he had been wounded by a police officer. Besides a slight wound of the scalp, he had been wounded by a sword in his back. The point of the sword was eight millimeters large; there was a transversal wound about one centimeter and a half (half an inch) between the ninth and tenth dorsal vertebræ, and three centimeters (an inch) from the line of the spinous processes. A physician, who had seen the patient at once, had introduced a stylet in the wound, and ascertained that its direction was oblique from the right to the left, and a little upwards. The patient complains of slight pains only near the wound. The lower limbs are completely deprived of voluntary movements. The next morning a better examination is made; the patient has not slept; he has suffered violent pains, principally in the left lower limb; he feels a kind of burning and numbness as if he were receiving electric shocks. The sensibility of the *left* lower limb is quite evidently increased. When a hand is simply applied upon this limb, the pains become very acute, and the very least pressure makes him shriek out. This morbid state of sensibility exists in the whole length of the limb, and also upon the left side of the sacrum and coccyx, and the upper and anterior part of the thigh. Higher up sensibility is normal. Even cold air, when the sheet is drawn down, causes pain in the left lower limb. Voluntary movements are impossible in all this limb, except in the toes, which may slightly move.

The *right* lower limb has a diminution of sensibility; the patient knows when he is touched, but when pricked with a pin he does not feel pain, and he does not distinguish a pressure by the finger from the pricking of a pin. In both cases he has only a sensation of contact. This limb is not deprived of movement as it was on the previous day. The flexion of the foot on the leg, and of the leg on the thigh, are executed; the movements are extensive, but the patient cannot altogether lift up his limb from the bed.

The temperature of the lower limbs is the same as that of the rest of the body, and there is no difference between them. All the organic functions are in a normal condition, except that there is a retention of urine and of the fæcal matters. Voluntary movements and sensibility are not altered in the abdomen, and all the upper parts of the body. In the afternoon the hyperæsthesia has gone a little higher on the left side in the upper parts of the abdomen, and the genital organs have also become very sensitive.

When a cloth that has been dipped into water at 30° (probably centigrade, 86° Fahr.), is applied to the left limb, the patient has a feeling of burning, which makes him cry out. When the cloth has been dipped into water at the low temperature of the room, the patient has a very acute feeling of cold.

On the right limb the wet cloth does not give either a sensation of warmth or cold, or of dampness or dryness, although he feels he is touched. The tickling of the right foot is not felt as tickling, and only as a *contact*. On the left foot tickling is exceedingly painful.

Gradually this patient became more and more able to move the right limb, and partly also the left limb. The hyperæsthesia diminished, particularly in the upper parts of the left limb; but the right limb became, for a time, unable to feel the contact of a hand, and if pricked there was a sensation, but the patient did not know its place. On the 20th of February, a slough was found on the right side of the sacrum; the patient had not felt anything there. In April, voluntary movements had returned in the two limbs, but sensibility was still deficient in the right one. On the 15th of June, the patient could walk with the help of a cane, and he left the hospital, not having yet, however, recovered entirely the power of feeling in his right limb.

Three years afterwards the patient was seen again, and he stated then that he was quite well, and that he could walk without difficulty or fatigue; but a year later, having walked a distance of many leagues, he found a large scar, produced, he said, by the friction of his pants on his right knee; he had felt no pain, and was surprised when he found this wound. Although sensibility was still deficient in this limb, all its movements were executed freely, and without fatigue. (Viguès, in '*Moniteur des Hôpitaux*', Sept. 3d, 1855, p. 838.)

After the cases of alteration of a lateral half of the spinal cord, Dr. Brown-Séquard relates cases of alteration of a lateral half of the medulla oblongata, pons varolii, &c.—cases offering proof of the decussation of the conductors of sensitive impressions in the spinal cord from another point of view. This case is one of these, and a very important one:

CASE 38.—S. G.—, æt. 28, was, on the 14th of May, suddenly seized with an acute pain in the right side of the head, and fell down in a state of insensibility, remaining so for twenty-four hours. On recovering, she found she had lost the power of moving her *left* arm, and, in a great degree, that of moving the leg of the same side. The right side was unaffected, except the face, the muscles of which were paralysed; those of the left side of the face retained their power. Sensibility of the *left* side of the body was destroyed, and likewise that of the right side of the face. She could not hear with the right ear. The right eye became inflamed several weeks before her death, and the cornea was slightly ulcerated; the upper eyelid was constantly raised. Her muttering was scarcely intelligible; paralysis of all the parts affected became complete; deglutition and mastication performed with great difficulty.

Autopsy, twelve hours after death.—A fibrous, semi-cartilaginous tumour was found on the *right* side of the tuber annulare and the medulla oblongata, seated in the substance of the dura mater and other membranes. It extended from the point where the fifth pair of nerves arises from the tuber annulare, covered the origin of this nerve and the whole of the right side of the tuber below this, and passed down along two thirds of the medulla oblongata, and adhered to the right side of the basilar artery. The right vertebral artery was enclosed in the tumour, which was about two inches long. The surface

of the root of the right crus cerebelli on which it pressed was softened, as was also that part of the tuber annulare on which it lay. It was incorporated with the substance of the right side of the medulla oblongata, and had produced softening as far as it reached. This softening extended through the posterior tract, but became less as it approached the posterior surface. The anterior tract was a pulpy mass. Neither the anterior nor the posterior tract of the left side was perceptibly affected. The tumour pressed upon the roots of the fifth, seventh, eighth, and ninth pairs of nerves. (S. Annan, in the 'American Journal of the Medical Sciences,' vol. ii, July, 1841, p. 105.)

Upon this case Dr. B.-Séguard says—

“We think that the case of Dr. Annan, if we take into account the extent of the injury in the medulla oblongata, bears out clearly that most if not all of the conductors of sensitive impressions from the trunk and limbs make their decussation in the spinal cord. But the importance of this case is not limited to this demonstration: it shows at once the radical difference between the symptoms of an alteration of a lateral half of the medulla oblongata above the crossing of the pyramids, and an alteration of a lateral half of the spinal cord either in the cervical or in another region. In this last case, as we have shown a moment ago, there is loss of movement in one side and loss of sensibility in the opposite side; while in a case of alteration above the crossing of the pyramids, we find that the loss of movement and of sensibility are both in the opposite side. This case is also excellent to show that the functions attributed to the restiform body as a conductor of sensitive impressions, and to the cerebellum as either a centre of perception of these impressions, or as a regulator of our voluntary movements, or as a centre for the guiding sensation, whether by a reflex action or otherwise, are not performed by these parts, as the communication between the cerebellum and the right side of the body through the right side of the medulla oblongata was almost impossible, a small part of the right restiform body remaining, and sensibility and voluntary movements being preserved in this side.”

Lecture VIII.—This lecture contains the conclusions to be drawn from the pathological cases related in the preceding lectures, as regards the diagnosis of alterations in the various parts in the spinal cord. Leaving aside the movements of the heart and lungs, the state of the sphincters, and of animal heat and nutrition, the groups of symptoms existing in disease of the spinal cord, according to the situation and extent of that disease, are thus summed up:

“1st. *Deep alteration of the posterior columns in all their length.*—Increased sensibility in the trunk and limbs for impressions of touch, for those due to pricking, pinching, and galvanic excitations, and for changes of temperature (cold and heat). Loss, or a very great diminution, of reflex movements. All kinds of voluntary movements possible, and more or less easily executed when the patient is in bed. Walking and standing very difficult.*

* On account of the loss of reflex action, and of the morbid sensibility, and also on account of the alteration in the *guiding sensations* coming from muscles—an alteration which is due to two causes, one of which is the loss of action of some of these conductors altered in the posterior columns through which they pass before reaching the gray matter, while the other is the morbid increase of sensibility of those conductors which go directly into the gray matter.

"2d. *Deep alteration of the posterior columns in the extent of the cervico-brachial swelling.*—Increased sensibility in the four limbs, and in the trunk, for all kinds of impressions. Diminution of reflex actions in the upper limbs, and increased reflex actions in the lower limbs. Some difficulty in the direction of the movements of the upper limbs, without the help of the sight. Standing and walking possible without any great difficulty.

"3d. *Deep alteration of the posterior columns in the extent of the dorso-lumbar swelling.*—Increased sensibility in the lower limbs, and normal sensibility in the upper ones. Diminution or loss of reflex actions in the lower limbs. Movements of lower limbs possible, and even easy, when the patient is in bed; but walking and standing very difficult.

"4th. *Deep alteration of a very limited part of the posterior columns.*—Increased sensibility, and increased reflex action, in all parts receiving their nerves from the spinal cord below the alteration. Voluntary movements possible and easy everywhere. The place of the alteration may be detected by diminution of reflex actions in the zone round the body receiving nerves from the level of the part altered in the posterior columns.

"5th. *Alteration of the posterior columns and posterior roots of the spinal nerves.*—Instead of hyperæsthesia, as in the preceding cases, diminution or loss of all kinds of sensibility, in places receiving the spinal nerves, continuing the roots which are altered. Voluntary movements still possible, in bed, and while the patient looks at his limbs, but walking and standing almost impossible. Reflex actions *completely* lost in all the anæsthetic parts. If the alterations are in the upper parts of the spinal cord, the other parts being healthy, then voluntary movements in the lower limbs, and even walking or standing, are possible, and may be easy, and these limbs have an increased sensibility and increased reflex actions.

"6th. *Alteration of the posterior columns and of the gray matter in all their length.*—There is no difference between this case and the preceding, except that here there is a real paralysis of voluntary movements, which is complete if the alteration extends to the anterior gray cornua. Greater frequency of formication and other sensations referred to the periphery.

"7th. *Alteration of the posterior columns and gray matter in any limited part of the spinal cord.*—Very nearly complete loss of sensibility. Degrees of paralysis of voluntary movements varying with the place occupied by the alteration in the length of the spinal cord. Reflex actions increased in parts receiving their nerves from the parts of the cord below the seat of the alteration.

"8th. *Alteration limited to the gray matter.*—The same symptoms as in the preceding cases, except that at first there is a greater degree of anæsthesia than of paralysis, if the alteration begins in the very centre of the cord. Formication and other sensations referred to the periphery, in cases of inflammation.

"9th. *Alteration of the anterior columns in the upper part of the cervical region.*—No paralysis, no anæsthesia, very slight hyperæsthesia, various sensations, particularly pain, referred to several parts of the body.

"10th. *Alteration of the lateral columns in the upper part of the cervical*

region.—Paralysis of voluntary movements in the four limbs and the trunk. Increased sensibility and greatly increased reflex actions in the paralysed parts.

“11th. *Alteration of the anterior columns in any part of their length, except the neighbourhood of the medulla oblongata.*—More or less complete paralysis of voluntary movements in all the parts receiving their nerves from or below the parts of the cord where the alteration exists. Slight hyperæsthesia. Reflex actions very much diminished in the parts which receive their nerves from the altered portion of the cord, and increased below these parts.

“12th. *Alteration of the lateral columns in any part of their length, except the neighbourhood of the medulla oblongata.*—Incomplete paralysis of movements. Hyperæsthesia. Diminution of reflex actions less than in the preceding case.

“13th. *Alteration of the anterior half of the spinal cord, including the anterior columns, a good part of the gray matter, and a part of the lateral columns.*—Voluntary movements completely paralysed. Sensibility very much diminished. For reflex actions, as in 11th.

“14th. *Alteration of the various parts of the spinal cord, except the posterior columns.*—Loss of voluntary movements and of all kinds of sensibility. Reflex actions increased or diminished in certain parts of the body, according to the place of the alteration in the length of the spinal cord.”

Lecture IX.—This lecture is on physiological and morbid actions due to the great sympathetic nerve. Respecting this nerve it is said—first, that it is essentially (though not exclusively) a motor nerve of blood-vessels; secondly, that it originates chiefly from the cerebro-spinal axis; thirdly, that its paralysis is characterised by a dilatation of blood-vessels and an afflux of blood, with the results of this afflux—increase of vital properties; fourthly, that its excitation, direct or reflex, is characterised by a contraction of blood-vessels, and the results of this contraction—decrease of vital properties.

An important question is proposed and answered in this lecture: it is—can we explain all the phenomena, normal and abnormal, which show the direct or the reflex influence of the nervous system on nutrition and secretion, by the above notions concerning the effects of paralysis or excitation of the sympathetic nerve on blood-vessels? “For several years,” says our author, “I have felt inclined to admit the possibility of an explanation of these phenomena founded only upon these notions, but I must say, that facts discovered by Ludwig, by Czermak, and, especially, by Professor Bernard, seem to have solved the question in the most positive manner, and that it seems absolutely certain that there is some agency of the nervous system which is not simply an influence on the constricting muscular fibres of the blood-vessels, in the normal or pathological phenomena of nutrition and secretion. I must add, also, that the views held by the most eminent British physiologists (Mr. J. Paget, Dr. Carpenter, Dr. Todd, and others,) have, by the discovery of the facts I allude to, received a sanction which, I confess, they needed. The principal amongst these facts, is the following: Instead of contracting, the blood-vessels of the salivary glands become enlarged, when certain nerves are excited. I think that this enlargement in the blood-vessels must be due to a greater

attraction of the arterial blood by the tissue of the gland; and we explain this increased attraction by the production of the chemical interchanges between the secretory tissue and the blood, which are rendered manifest by the secretion of saliva then taking place.

"The researches of Czermak and of Professor Bernard tend to show that the increase in the salivary secretion does not depend on the sympathetic nerve, but on the lingual; and we have now, in this discovery, the explanation of this apparent contradiction: how can it be that the glands of the eye, of the ear, &c., secrete more when their blood-vessels are paralysed and enlarged after the section of the sympathetic nerve, and that an increase in the secretion of the salivary and other glands is due to a nervous excitation? How can it be that, in one case, secretion is increased when the blood-vessels are dilated, and that in other cases it would be increased, while their vessels ought (according to what we thought) to be contracted? Bernard shows that, instead of being contracted, they are dilated; and, besides, the experiments of Czermak and Bernard show that the salivary secretion is arrested when the sympathetic nerve is excited; and we know that this nerve, when excited, has the same influence on the lachrymal and on the mucous glands of the eye and ear, &c.

"From this discussion we conclude that there are two kinds, at least, of immediate influences of the nervous system, either by a direct or by a reflex action, on nutrition and secretion, normal or pathologic. By one, which we see plainly when the cervical sympathetic nerve is excited, the blood-vessels contract, and there is a diminution of secretion and nutrition; by the other, the discovery of which is chiefly due to Professor Bernard, the blood-vessels dilate in consequence of a greater attraction for arterial blood developed in the tissues. Which of these two modes of action is the more frequent? and which is the most powerful in producing the normal and the morbid phenomena of nutrition and secretion?"

Lecture X.—This treats, at considerable length, of the influence of the nervous system upon nutrition, secretion, and animal heat; with remarks on the importance of the knowledge of this influence for the diagnosis and treatment of disease. With respect to the supposed existence of a system of *excito-secretory* or *secretory* nerves, which has been recently claimed as a new discovery by Dr. H. F. Campbell, of Georgia, and by the late Dr. Marshall Hall, Dr. Brown-Séquard says—"that Dr. Campbell seems really to have been the first to introduce in science the hypothesis that there exists a secretory or excito-secretory system of nerves, but that neither he nor Dr. Hall has adduced a *single* fact to prove the existence of this pretended independent or distinct *system of nerves*. Both these physiologists seem not to have been aware that reflex secretions and reflex changes in nutrition were perfectly known, and that the question was, not to prove that there are such reflex phenomena, but whether they are to be explained by a reflex influence on blood-vessels or otherwise. Any one desirous to know the state of science, in this respect, before the first publication of Dr. Campbell, will find, easily, that it was more advanced than in the last paper of this able physiologist, in Müller's 'Manual of Physiology' (2d German edition, 1837), in Stilling's treatise on 'Spinal Irritation'

(1840), and in several works of Henle published in 1840 and 1841. Since that time there has been no treatise on Physiology or General Pathology, and no paper nor other work on Inflammation, that does not speak of reflex phenomena of nutrition or secretion as of something well known."

Lecture XI.—This lecture is upon the etiology, nature, and treatment of epilepsy, with a few remarks on some other affections of a similar character. It is a short summary of the author's separate work on epilepsy, which will be found noticed in the next article.

Lecture XII.—This lecture, which is not yet printed, is upon the medulla oblongata, the pons-varolii, and some parts of the spinal cord, in their relations with respiratory movements, with vertiginous or rotatory convulsions, with the transmission of sensitive impressions and of the orders of the will to the muscles, and with the vaso-motor nerves and animal heat.

Researches on Epilepsy; its artificial production in animals, and its etiology, nature, and treatment in man. First part of a new series of experimental and clinical researches applied to physiology and pathology. By E. BROWN-SÉQUARD. (8vo, Boston, David Clapp, 1857, pp. 82).

Eight years ago, Dr. Brown-Séquard discovered that certain injuries of the spinal cord in mammals, and particularly in guinea-pigs, were followed by a convulsive affection resembling epilepsy. This strange result was brought about by a complete transversal section of a lateral half of this organ—a transversal section of the two posterior columns, of the posterior cornua of gray matter, and of a part of the lateral columns—a transversal section of either the posterior or lateral or anterior columns simply—a complete transversal section of the entire organ—a simple puncture. The convulsive affection was produced with most certainty when the part injured was situated between the seventh or eighth dorsal and the third lumbar vertebræ; the time at which it was produced was generally in the course of the third or fourth week after the injury.

At first the spasms are limited to the face and neck, but in a few days they might extend, and usually did extend, to all the non-paralysed parts—even to the intestine, bladder, and seminal vesicles. Sometimes the fit is ushered in by a cry. Usually the consciousness is annihilated during the fit, but occasionally the animal will cry at this time when it is irritated. Usually the fits occur in series, each paroxysm lasting two or three minutes, and the intervals between one and another being of about the same length. Generally the convulsions are followed by more or less drowsiness. Generally, also, the respiration is greatly impeded, and in the height of the paroxysm the lips and tongue are quite blue.

These convulsions may come on either spontaneously or after certain excitations. They come on when the breathing is arrested

for a very short time—much shorter than in an uninjured animal of the same kind. They come on also (and this is perhaps the most curious part of the discovery) upon pinching the skin in certain parts of the face and neck. These parts are on the same side as that on which the spinal cord has been injured, and if the cord has been cut through entirely they are on both sides. “No other part of the body but a portion of the face and neck has this faculty. In the face, the parts of the skin animated by the ophthalmic nerve cannot cause the fits; and of the two other branches of the trigeminal nerve, only a few filaments have the property of producing convulsions. Among these filaments, the most powerful in this respect seem to be some of those of the sub-orbital and of the auriculo-temporalis. A few filaments of the second, and perhaps of the third cervical nerve, have also the property of producing fits. In the face the following parts may be irritated without inducing a fit: the nostrils, the lips, the ears, the skin of the forehead and that of the head. In the neck there is the same negative result when an irritation is brought upon the parts in the neighbourhood of the median line, either in front or behind. On the contrary, a fit always follows an irritation of some violence, when it is made in any part of a zone limited by the four following lines: one uniting the ear to the eye; a second from the eye to the middle of the length of the inferior maxillary bone; a third, which unites the inferior extremity of the second line to the angle of the inferior jaw; and a fourth, which forms half a circle from this angle to the ear, with the convexity approaching the shoulder” (p. 6).

It is in the cutaneous ramifications of the nerves belonging to this part, and not in the trunks of these nerves, that this curious faculty of producing convulsion belongs. If the trunks of these nerves be laid bare and irritated, there is no result, but a very slight irritation is sufficient to bring about an attack when it is applied to the extremities of these nerves. What the nature of the change in these nerves may be is not very apparent. It is not increased sensibility to pain, for, after a fit, it is easy to see that there is no such increased sensibility. Besides, if this were the cause, it is to be expected that a fit would be brought on (which is not the case) by irritating a part which is at first in a very exalted state of hyperæsthesia, and that is the hind limb on the side upon which the spinal cord has been cut through. Dr. Brown-Séquard is of opinion that the change which has passed over the nerves of the part in question, is confined to those twigs which minister simply to reflex movements—to twigs which have no concern with ordinary and appreciable sensations; and that the quasi-sensation which is propagated from the ramifications of the nerves to the nervous centres in causing the fit is analogous to the *aura epileptica*; but, be the explanation what it may, the fact remains. It is, also, a fact, which has yet to receive its explanation, that the part in question is very much infested with lice. It would even seem as if these creatures left the rest of the body in order to congregate there. It would seem also as if the part were somewhat more vascular than usual, though this may be an effect of the pinching to which it is subjected.

These facts lead to many interesting conclusions. 1st. They give a positive proof that an injury to the spinal cord may be the cause of an epileptiform affection. 2d. They point out the existence of a very curious relation between certain parts of the spinal cord and certain branches of some of the nerves of the face and neck. 3d. They show that epileptiform convulsions may be the constant consequence of slight irritation upon certain nerves. 4th. They show that in an epileptiform affection, even when it has its primitive cause in the nervous centres, some cutaneous ramifications of nerves, not directly connected with the injured parts of these centres, have a power of producing convulsions, which other nerves, even directly connected with these centres, have not. And 5th. They show that the cutaneous ramifications of certain nerves may have the power of producing convulsions, while the trunks of these nerves have not this power.

— After stating these facts, Dr. Brown-Séquard proceeds to use them as an additional argument for supposing that epilepsy may originate from alterations of the spinal cord, and then he adduces a considerable amount of clinical evidence as to the frequent existence of such alterations in epilepsy. He does not hold, however, that the spinal cord is the exclusive seat of the disease. On the contrary, he tries to show that the disease may originate in any part of the nervous system. In reviewing the symptoms, also, he shows that no single symptom belongs exclusively to epilepsy connected with disease of the brain, or with disease of the spinal cord, or with disease of a nerve, and that the existence of the *aura epileptica* is no proof that the primitive cause of the disease is in some cutaneous nerve, and not elsewhere. Thus, in the case of the epileptic animal, it might be supposed that the primitive cause of the disease was in the nerves ramifying upon and near the angle of the jaw, if it were not known that there was beyond and before this a central cause of mischief in the spinal cord.

From what occurs in animals after an injury to the spinal cord, and from some cases observed in man (some of which cases are cited), Dr. Brown-Séquard infers there is not unfrequently in epileptic patients some particular spot the irritation of which will give rise to a fit. This irritation may not be attended by any positive sensation, and it may be difficult to find, but when it is found, the object of treatment will be to prevent the transmission of the irritation from it to the nervous centres by ligatures, by section of the nerves, by cauterization, and so on. Great stress, indeed, is laid upon the *aura epileptica*, and this phenomenon is raised from the grade of a symptom to that of a cause. The *aura* is indeed looked upon as identical with that impression which is transmitted from the neighbourhood of the angle of the jaw in the epileptic guinea-pigs, and because the fits of these animals may often be prevented by dividing the nerves proceeding from this spot, or by cauterizing their extremities, it is thought to be of great importance to interrupt the course of the *aura* by ligatures, or by dividing the nerves, or else to destroy it in its source by cauterizing the extremities of these nerves. And several cases are cited from various authors,

in which epilepsy would seem to have been relieved or cured in this manner.

Then, after devoting several pages to a critical examination of several theories of epilepsy, Dr. Brown-Séquard proceeds to state his own views respecting some of the principal questions in connexion with the theory of epilepsy.

The brain, he shows, is not essential to the production of epileptiform convulsion, nor yet the cerebellum. He shows this by experiment upon his epileptic animals, for convulsions were still produced (though weaker—probably from the loss of blood) by pinching the neighbourhood of the angle of the jaw, after removing the whole of the encephalon except the medulla oblongata and pons varolii. There are, he thinks, three distinct elements wanting for the production of a fit :

“1st. Increase of the *force* of the reflex property.

“2d. Increase of the *excitability* of this property.

“3d. An excitation of a special nature, or a very violent one” (p. 59).

The paleness of the face, which, according to Drs. Delasiauve, Trousseau, and Radcliffe, is a primary phenomenon in the fit, is used as a key by which the remaining phenomena are accounted for. “After Claude Bernard had discovered that the section of the cervical sympathetic nerve is followed by a dilatation of the blood-vessels of the face, I found,” says Dr. Brown-Séquard, “that when this nerve is irritated by galvanism, there is a contraction of these blood-vessels, and I explained the facts discovered by the eminent French physiologist and myself by considering the sympathetic as the motor nerve of the blood-vessels of the face. I found also that the branches of the sympathetic nerve which animate the blood-vessels of the face originate from the spinal cord, with the branches of the same nerve going to the iris. The theory I then proposed has been almost universally admitted. We have in this theory an easy means of explaining the paleness of the face in epilepsy. When the excitation takes place in the spinal cord and in the base of the encephalon, which gives rise to the fit, the nerve-fibres which go to the head are irritated, and produce a contraction of its blood-vessels. Of course this contraction expels the blood, and, in consequence, the face becomes pale. Very often another effect, depending on the nerve-fibres of the cervical sympathetic, is produced—the dilatation of the pupil. But the reverse sometimes takes place—a contraction of the pupil occurring instead of a dilatation. This last phenomenon is easily explained by admitting that the excitation in the nervous centres takes place near the origin of the third and fifth pair of nerves, and not of that of the cervical sympathetic, as in the case where that pupil dilates.” (pp. 61, 62.) * * * *

“We think that at nearly the same time when the origin of the branches of the sympathetic nerve going to the blood-vessels of the face receive an irritation in the beginning of a fit of epilepsy, the origin of the branches of the same and of other nerves, going to the blood-vessels of the brain proper, also receive an irritation. A contraction then occurs in these blood-vessels, and particularly in

the small arteries. This contraction expelling the blood, the brain proper loses at once its functions, just as it does in a complete syncope. Now, as it has been well proved by the researches of Kellie, of Abercrombie, of John Reid, of Henle, and of Foltz, that the quantity of liquid in the cranio-spinal cavity cannot change suddenly, it results, that if there is less blood in the brain proper, there must be more in the basis of the encephalon and in the spinal cord. In consequence of the impediment to respiration, the blood sent to the encephalon, as well as to other parts of the body, contains but little oxygen, and is charged with carbonic acid, so that the large quantity of blood accumulated in the basis of the encephalon (the medulla oblongata, the pons varolii, the tubercula quadrigemina, &c.) and in the spinal cord, is endowed in a high degree with the power which I have shown that such blood possesses, *i. e.*, to excite convulsions. It may be, as Henle has supposed, that the basis of the encephalon is also excited to cause convulsions in consequence of the pressure exerted upon it by the accumulation of blood. The spinal cord, also, in all its length, is then excited to produce convulsions by the blood which circulates in it." (pp. 62, 63.)

Again: "In the same way as there are *certain* muscles that contract in the neck, in the larynx, and elsewhere, we may admit that there are *certain* blood-vessels that contract either in some parts of the brain proper or in the nervous portions of the organs of sense, and, in consequence, there is a trouble or loss of either one or several senses, or of the intellectual faculties, consciousness remaining more or less entire; or there is a successive loss of sight, of hearing, of the intellectual faculties, and, last of all, of consciousness." (p. 66.)

The augmentation of the reflex excitability, which is regarded as a principal element in epilepsy, is looked upon as the direct effect of the increased nutrition of the nervous centres concerned, and this increased nutrition is thus accounted for: "An excitation on some part of the nervous system causes a contraction of the small blood-vessels of a part of the cerebro-spinal axis, and as the same quantity of blood still arrives by the various arteries in the cerebro-spinal cavity, it results that if the small ramifications of some arterial branches are contracted, the others receive more blood, so that nutrition, and, in consequence, the reflex excitability, augment in the parts to which they are distributed. But this is not likely to be the most frequent mode of increase of nutrition. We have found that when a vascular nerve is excited for a long while, the contraction of the blood-vessels after a certain time ceases, and a dilatation takes place, which lasts longer than the contraction, though the nerve is still excited; this is paralysis by excess of action. In the nervous centres very likely this paralysis of the blood-vessels supervenes also after considerable contraction, and, in consequence of this paralysis, nutrition is increased in the parts of these centres where it exists, as we have found that nutrition is increased in the nerves and muscles of the face, when the blood-vessels are paralysed." (p. 69.)

With respect to the *treatment* of epilepsy, only a few propositions

are laid down. The first thing is to find out whether the origin of this disease is peripheric; and if it is, then proper means are to be employed to separate the point of origin from the nervous centres (ligatures, laying bare the nerve and etherizing it, or, if this fails, dividing it), or else to eradicate the point of origin by various modes of cauterization. "The best means of treating epilepsy seem to consist in the application of a series of moxas along the spine, and particularly the nape of the neck." Again: "The nutrition of the nervous centres may be modified, and thereby epilepsy be cured, principally by medicines which act on the blood-vessels, such as strychnia, but particularly by those which determine contraction in those vessels, such as atropia, ergot of rye, &c." Cauterization of the mucous membrane of the larynx is spoken of as an excellent means, not only of counteracting spasm in the larynx, but of modifying the nutrition of the medulla oblongata. As a means of cure, also, Dr. Brown-Séquard thinks "it would be of the utmost importance to have fever and ague generated in epileptics." Why, he does not very clearly explain (p. 82). And lastly, hygienic means are regarded as of equal importance to medicines, and it is laid down that sleeplessness is to be combated as uncompromisingly as the disease itself. In order to shorten the fit, and in this way diminish the dangers resulting from the circulation of black blood in the nervous centres, the best means are said to be—dashing cold water in the face, and the inhalation of chloroform.

—Such is the gist of this very remarkable and very important book—a book which is a mine in which physiologists and pathologists may dig for years to come without exhausting it of its rich materials.

On Epileptiform Convulsions after bleeding, and upon Epileptic Convulsions generally. By Professors KUSSMAUL and TENNER. ('Untersuch. z. Naturlehre der Mensch u. d. Thiere,' von J. Moleschott, 2d vol., Frankfort, 1857.)

In this memoir the authors endeavour to interpret some of the principal phenomena of epileptic and epileptiform convulsions, and the conclusions at which they arrive are of very great importance. In several points the authors trench very closely upon grounds already occupied by Drs. Brown-Séquard, Radcliffe, and others, but they do not appear to have been aware of this fact. The conclusions themselves, which are based in many instances upon original experiments, are as follows:

1. The convulsions which are produced by bleeding in man and warm-blooded animals generally, are similar to those which happen in epilepsy.

2. Convulsions of the same kind are produced when the supply of red blood to the brain is suddenly cut off by tying the arteries in the neck.

3. Epileptiform convulsions are also produced when arterial blood acquires a venous character, as after placing a ligature around the windpipe.

4. It is extremely probable that the convulsions are produced in these several cases by some sudden interruption to the nutrition of the brain, and not by any mere change in the pressure proceeding from the blood-vessels.

5. The epileptiform convulsions, produced by hæmorrhage, do not proceed from the spinal cord.

6. Nor do they proceed from the great encephalic masses.

7. They proceed rather from the excitable portions of the brain which lie behind the optic thalami.

8. Anæmia of those parts of the brain which are anterior to the crura cerebri in man gives rise to loss of consciousness, insensibility, and paralysis; and if convulsions are superadded, there must be some change in the excitable parts which lie behind the optic thalami.

9. Anæmia of the spinal cord gives rise to paralysis of the limbs and chest and neck, and occasionally, but not frequently, if the blood is abstracted suddenly, the paralysis is preceded by tremulous movements in the limbs.

10. The convulsions of hæmorrhage are not due to a moral cause, nor are they the effect of reflex action.

11. Hæmorrhage is not attended by convulsions in cold-blooded animals (at least, not in frogs), or in cases where the blood is abstracted very slowly, or in very feeble animals, or in cases where the nutrition of the spinal cord has suffered, or where large portions of the brain have been removed, or in etherized animals, or, in some cases, where the excitable portions of the brain have become diseased in certain ways.

12. Etherization hinders the outbreak of convulsions in asphyxia; consequently the state of etherization and asphyxia must be considered as in some degree antagonistic.

13. The brain of warm-blooded animals very soon loses the power of resuming its vital action when the supply of arterial blood is cut off, and in this way apparent death rapidly changed into actual death. In a rabbit the supply of arterial blood may be cut off from the brain for two minutes without killing the animal, but not for a longer time.

14. After tying the arteries of the neck it sometimes happens that *rigor mortis* seizes upon the muscles of the trunk and limbs, before the irritability of the left side of the heart is at an end, and therefore, the left heart cannot be regarded as the *primum moriens* among the muscles.

15. Contraction of the pupils followed by extreme dilatation in the mortal agony is not, as Bouchat believes, a certain sign that life cannot be prolonged.

16. The return of red blood is the best cure for convulsions which have been produced by hæmorrhage.

17. Antiphlogistic measures, and especially bleeding, ought never to be employed in the treatment of epilepsy.

18. It can be demonstrated by experiment, that the quantity of blood within the skull in living animals is capable of considerable variation.

19. The quantity of blood within the skull is increased when the arterial stream is allowed to return by the removal of ligatures from the great arteries of the neck (arterial congestion); when the veins of the neck are tied (venous congestion); when the cervical prolongations of the sympathetic are divided (arterial and venous congestion); and when the trachea is tied during inspiration (venous congestion in asphyxia).

20. The quantity of blood within the skull is diminished by hæmorrhage, by tying the cervical arteries (passive anæmia), as well as by irritating with galvanism the vaso-motor nerves of the head (active anæmia).

21. There is more blood within the skull after tying the arteries than after hæmorrhage. The diminution in the quantity of blood is observed equally in the small arteries and veins and in the capillaries.

22. It is seldom possible after death to draw a just conclusion as to the quantity of blood contained within the skull, for the circulation is modified by many circumstances which act during the agony, and even after death.

23. The phenomena of the *petit mâl* derive their origin from changes in the brain proper; those of the *grand mâl* from alterations in the entire encephalon. The convulsions of epilepsy are, with justice, referred to the brain; and the spinal cord, in all probability, has only to play the part of a conductor in respect of them.

24. The anatomical changes in the brain, to which attention has been directed, can only be regarded as predisposing causes of epilepsy. Certainly they are not proximate causes.

25. Pathological anatomy conducts us to no conclusion with respect to the nature of epilepsy.

26. The sudden alteration of nutrition in the brain, to which the attack may be referred, is only manifested at the moment of the attack.

27. Arterial congestion of the brain does not appear to cause other than the phenomena of paralysis (vertigo and apoplexy).

28. Venous congestion of the brain, like arterial and venous congestion, gives rise to a condition in which epileptiform phenomena precedes the apoplectic—a condition in which the glottis is paralysed, and the respiration considerably retarded.

29. It is not the sphagismus or trachelismus of M. Hall, to which the epileptic convulsion can be referred, but rather the laryngismus. Every theory which refers the convulsion to a sudden afflux of blood, whether active, passive, or mixed, is false.

30. In all probability there are certain forms of epilepsy which are confined to convulsive movements in the muscular walls of the cerebral vessels.

31. The condition which predisposes to the epileptic attack is met with, sometimes in the encephalon as a whole, sometimes in a part of this organ only.

32. The medulla oblongata, inasmuch as it is the point of origin of the constrictor nerves of the glottis, and of the vaso-motor nerves, would seem to be the most ordinary source of mischief in epileptic and convulsive attacks.

Epilepsy and other convulsive affections, their pathology and treatment. By C. B. RADCLIFFE, M.D., F.R.C.P., Physician to the Westminster Hospital, &c. (Second edition, revised and enlarged, post 8vo, London, Churchill, 1856, pp. 383.)

As in the former edition of this work, the author introduces the remarks he has to make upon epilepsy and other convulsive disorders by certain considerations respecting the physiology of muscular motion. In these considerations he endeavours to establish three propositions, viz.:

1. That muscular contraction is not produced by the stimulation of any property of contractility belonging to muscle.

2. That muscular elongation is produced by the simple physical action of certain agents, electricity and others, and that muscular contraction is the simple physical consequence of the cessation of this action.

3. That the special muscular movements which are concerned in carrying on the circulation—the rhythm of the heart, and the movements of the vessels which are independent of the heart—are susceptible of a physical explanation when they are interpreted upon the previous view of muscular action.

In establishing these propositions the former line of argument is greatly changed, and much new matter is introduced.

— In treating of epilepsy and other convulsive affections, the author does not propose to enter into every part of the subject. On the contrary, he passes by several topics of considerable interest in themselves, but only of secondary importance in the argument, because he does not wish to divert attention from the main object he has in view, which is, to point out the necessity for a fundamental change in the pathology and treatment of the disorders under consideration—a change which is altogether in accordance with that which would seem to be demanded by the physiology of muscular action.

Dr. Radcliffe looks upon epilepsy as the great type of convulsive disorders, and the key to their interpretation. Epilepsy is, however (he tells us), a name which indicates much less than it did formerly. Thus, it does not indicate the epileptiform convulsion which is connected with certain positive diseases of the brain, with fever, with certain suppressed excretions, with “irritation” in the gums and elsewhere, or with the moribund state; and it is difficult to say what it does indicate, for as our diagnosis gains in exactness, epilepsy changes more and more from a special malady into a mere symptom. At the same time, it is, and in all probability it always will be, convenient to take an ideal form of epilepsy and regard it as a special malady, for there are, and ever must be, numberless cases in which, in their earliest stages at least, it will be very difficult, if not impossible, to recognise the disease of which the convulsion is merely a symptom.

Passing from this ideal form of epilepsy to the consideration of the actual disorders in which muscular contraction is in excess, the

author divides these disorders into three categories, of which the distinctive signs are tremor, convulsion, and spasm.

In the first category, where tremor is the distinctive sign, he considers the tremors of delicate and aged persons, of paralysis agitans, of delirium tremens, the rigors and subsultus of fevers, and the tremblings of slow mercurial poisoning.

In the second category, of which convulsion is the distinctive feature, a division into two sections is made by the presence or absence of consciousness during the convulsion. Where consciousness is present, the convulsion is spoken of as simple; where it is absent, as epileptiform. Under simple convulsion he considers the convulsion which is met with in hysteria, chorea, and in those strange affections which take an intermediate position between the two, as the dance of St. Vitus and St. John, tarantism, and other affections of the kind. Under epileptiform convulsion he considers the convulsion connected with certain diseases of the brain—chronic softening, chronic meningitis, tumour, induration, hypertrophy, atrophy, congestion, apoplexy, inflammation—with fever, with certain suppressed excretions, with “irritation” in the gums and elsewhere, and with the moribund state.

In the third category, of which spasm is the distinctive feature, he considers catalepsy, tetanus, cholera, hydrophobia, ergotism, the rigidity of cerebral paralysis, the spasm connected with certain diseases of the spinal cord, and some other spasms of a minor character.

— In succession, the history of these several affections is carefully examined, and consequently, many things are added which have no place in the first edition. As with the considerations respecting muscular motion, the whole argument has been reconsidered, rewritten, and greatly amplified, and the work is offered to the reader rather as a new book than a second edition.

On malformations of the Human Heart, with original cases. By T. B. PEACOCK, M.D., F.R.C.P., Assistant-Physician to St. Thomas's Hospital. (Churchill, 8vo, pp. 143, 1858.)

The subjects treated of in this important essay are: congenital misplacements of the heart, deficiency of the pericardium, malformations, the mode of formation, the symptoms and effects, the diagnosis, and medical management. The malformations include those which are dependent on arrest of development at an early period of foetal life, those which prevent the changes which should ensue after birth, those which do not interfere with the functions of the heart but may lay the foundations of disease in after-life, and, lastly, those which consist in the irregular development of the primary vessels. Dr. Peacock does not allude to those forms of defect in the development of the heart which are incompatible with extra-uterine life, or which have only been met with in the lower animals. The essay, which is of much practical as well as literary value, is based upon 153 cases, six of them original, of various forms of decided and important malformation. Its practical value will be best tested, perhaps, by the following remarks upon the diagnosis:

“The detection of the existence of malformation of the heart in ordinary cases, when the patient is seen in early life, can scarcely present any difficulty. The statement that palpitation, dyspnœa, and more or less cyanosis, had existed since birth, or shortly after, and the evidences of obstructed circulation at the time of examination, render the case sufficiently clear. M. Louis, indeed, regards the occurrence of ‘suffocative attacks brought on by the slightest cause, often periodic, and always very frequent, and accompanied or followed by syncope, and with or without the blue discoloration of the body generally,’ as pathognomonic of communications between the right and left cavities of the heart; and the cyanotic discoloration, when present, can scarcely be mistaken. But the ordinary symptoms may be absent, or may exist only to a slight degree, or the patient may not be seen till after he has attained the age of puberty or manhood, and there may be no satisfactory history of his previous state of health to aid the diagnosis. Though, in cases of this kind, if the patients had been under medical care, it is quite possible that sufficiently characteristic signs might have been observed, we are sometimes assured by the patient and his friends that he had enjoyed good health, had been capable of following a laborious occupation, and had presented nothing unusual in his appearance, until shortly before the time at which he falls under our notice. In such cases, then, it may be extremely difficult to decide whether the patient labours under some form of malformation, or under ordinary disease of the heart; and the differential diagnosis can only be effected by a careful examination and analysis of the general symptoms and physical signs.

“In all cases, also, the detection of the precise form of malformation must be a task of considerable difficulty, and in some instances entirely impracticable. Where an infant suffers from great difficulty of breathing and palpitation, and is intensely and constantly cyanosed, at or immediately after birth, it may be inferred that it labours under some serious malformation occasioning great obstruction to the circulation of the blood, as obliteration or great contraction of the pulmonic orifice, or transposition of the aorta and pulmonary artery. On the contrary, when the symptoms do not manifest themselves at so early a period, and are less constant and intense, there is probably only some slighter malformation, as a moderate amount of contraction at the pulmonary orifice. Of 153 cases of various forms of decided and important malformation, of which I have collected notes, in 74 there existed more or less contraction of the orifice of the pulmonary artery, or other sources of obstruction to the exit of the blood from the right ventricle, and in 25 others the orifice or trunk of the vessel was obliterated. In those patients who survived the age of twelve, the entrance of the blood into the pulmonary artery was interfered with in a much larger proportion of cases, or in 32 out of 39; so that, in any given case of malformation, especially after the age of fifteen, the probability is that the pulmonary artery is contracted. If this be the case, a loud systolic murmur will be heard in the præcordial region, and most intensely at the level of the nipple, and between that body and the sternum. It will be audible very distinctly in the course of the pulmonary artery, or from the base of the heart towards the middle of the left clavicle; and less distinctly in the course of the aorta, or at the upper part and right side of the sternum. If the pulmonic orifice be

permanently open, as is often the case, especially where the whole of the valves are united, there may also be a diastolic murmur; but, from the very small size of the aperture in most instances, the regurgitant current is probably generally too slight to generate a distinct murmur. Most usually with considerable contraction of the pulmonary orifice, the septum of the ventricles is defective, and the aorta derives its supply of blood from both ventricles; and, if so, a systolic murmur may probably be produced by the meeting of the two columns of blood in the ascending aorta, which may modify the signs observed. Generally, in such cases, the aorta is unusually large, and, from the powerful reaction on the valves during the diastole of the heart, a loud ringing second sound is heard on listening at the upper part of the sternum. With these signs there will also be perceived those of hypertrophy and dilatation of the right ventricle and auricle, and frequently a distinct jugular pulsation will be observed. The heart being much increased in size, and its walls hypertrophied, the dull space will be extended beyond its usual limits, especially towards the right side. From the yielding of the parietes in early life, the præcordia is also generally prominent. The impulse of the heart is usually powerful, and frequently a distinct purring tremor may be felt over the situation of the pulmonic orifice. The pulse is generally quick, small, and weak. It has been thought that, in cases of obstruction at the right side of the heart, the patient is disposed to let the head hang down so as to compress the chest, rather than to adopt the upright position, which we most frequently see selected by patients with disease of the left orifices; but I have seen patients with aortic disease hang themselves completely over the side of the bed, so that this rule does not certainly apply.

“If the evidence of obstruction at the pulmonic orifice be tolerably conclusive, we may safely infer there is either a deficiency in the septum of the ventricles, or a patent foramen ovale, for one or other of these defects almost invariably coexists with that condition. An aperture in the septum of the ventricles, without other malformation, would probably be attended by a murmur, caused by the flow of blood through the abnormal opening from the left ventricle into the right ventricle or auricle. The detection, therefore, of a systolic murmur at the base of the heart, without signs of obstruction at the aortic or pulmonic orifice, might lead to a suspicion that such a communication existed. This surmise would be strengthened if the murmur were not propagated in the course of the pulmonary artery or aorta; and especially if the patient were long under notice, and constantly presented the sign, without other evidence of cardiac disease or defect; and without having had any disease or accident during life which could probably have produced such a change in the heart as would be likely to be attended by a permanent murmur.

“I do not know that there are any means of detecting the open state of the foramen ovale; and there are also other malformations of the heart, such as transposition of the aorta and pulmonary artery, which could not be at all diagnosed during life.

“In some cases, as where the ductus arteriosus or foramen ovale remains open, the diagnosis may be aided by ascertaining whether the infant has been born prematurely, or at the full period.

“It has already been mentioned that the malformations of the valves

do not necessarily entail any interference with the functions of the heart. When they lead to disease of the organ, the symptoms and physical signs will be those of valvular disease dependent on any other cause. I believe that when a patient in early or adult life labours under symptoms of valvular disease, more especially at the aortic orifice, without having previously sustained any severe injury or strain, and without having had any serious rheumatic attack, we shall generally be correct in inferring that the valves are malformed."

On Dropsy connected with Disease of the Kidneys (Morbus Brightii), and on some other diseases of these organs associated with albuminous and purulent Urine: illustrated by numerous Drawings from the Microscope. By W. R. BASHAM, M.D., F.R.C.P., Physician to the Westminster Hospital. (London, Churchill, 8vo. p. 241, 1858.)

"The microscope as an instrument of investigation," says Dr. Basham in his opening sentence, "should be to diseases of the kidney what the stethoscope is to diseases of the lungs. The ear detects, by the aid of the one, the alteration of the sounds of respiration induced by disease; the eye, assisted by the other, sees in the urine materials and products thrown off from the kidneys, which, when carefully studied, become safe and reliable exponents both of the nature of the disease, and of its advance and decline."

The question then is—can the microscope do this? Can it clear up these most important questions in diagnosis and prognosis? The answer of Dr. Basham is unhesitating, and that answer is, we think, unimpeachable—it *can*. The answer, moreover, is one which every one for himself, with an ordinary instrument and with ordinary skill in its use, may hope to attain to, and that easily.

Dr. Basham is of opinion that neither the estimation of the amount of albumen, nor the weight of the solid constituents of the urine, will supply the requisite data on which reliance can be placed for deciding with any degree of certainty as to the stage at which the renal disorder has arrived, or as to the rate at which it is progressing or receding.

"I freely confess," he says, "to great scepticism of the practical value (its relation to treatment) of minute attention to the specific gravity of the urine, from which some authors have sought to deduce the amount of solids daily excreted by the kidneys. Independent of circumstances which combine to make me doubt the efficiency of the ordinary method in use, by urinometer, for determining with anything like accuracy the specific gravity of the urine, I am inclined to advance one step farther in heterodoxy, and express my opinion that the specific gravity of the urine, however accurately obtained, must fail to afford any very certain or satisfactory information in relation to the progress of renal disease. There is no animal fluid subject to such hourly variation in regard to its density as the urine; so that unless all the urine of the twenty-four hours be collected no truthful result could be obtained. But even when this is done, no practical inference of any importance is obtained; for the specific gravity of the urine, both in health and disease, will vary one day with another, influenced by a variety of extraneous

circumstances: the temperature, the density of the atmosphere, its hygrometric condition, the quality of the food, the quantity of fluid drunk, the activity or torpidity of the alvine functions, the amount and nature of the exercise of the body generally;—these will severally influence the density of the urine. It appears almost certain that this great variation in the specific gravity of the urine is imperatively needed, that the blood may be maintained at a definite and fixed density. Physiologically, it is easily conceived what irregularity and disturbance of function would everywhere ensue if the blood were continuously undergoing variations in its degree of concentration which must occur under the varying circumstances just enumerated, if the kidneys were not, according to the vital requirements, perpetually engaged in regulating and maintaining the necessary equilibrium in the density of the circulating fluid. It is only thus that the endless variability in the specific gravity of the urine can be explained, or that we can account for the excess of saline constituents and urea at one period, and their absolute decrease with a positive augmentation of water excreted at another. If these variations be thus infinite in health, how much more irregular and ill defined may they not be in disease. An estimation, therefore, of the specific gravity of the urine leads to no practical result; the information it conveys has no absolute value, for from the density of the urine, as an isolated fact, no inference or conclusion in relation to prognosis or treatment can be drawn. It is not intended by these observations to declare that the density of the urine should not be taken into the account, when we are recording *all* the qualities of the urine, physical, chemical, and morphological, with the view of describing the conditions of this fluid significant of disease. It is only asserted that the specific gravity of the urine as an individual property affords of itself no trustworthy information either in relation to the progress of the disease or the success of the treatment. I do not forget that a pale lemon-coloured albuminous urine of low specific gravity indicates a stage of disease very different from what is inferred from urine deep-coloured, albuminous, and of high specific gravity. But these opposite states of concentration teach less than the microscopic appearances accompanying these different densities; and it is from these microscopic appearances that in reality we obtain the information we seek.

“The instrument ordinarily employed for taking the specific gravity of the urine—the urinometer—affords but the rudest approximation to the true specific gravity; where the object is only to ascertain if the urine be of *high* or *low* specific gravity, a watery urine, or a fluid highly charged with organic urinary products, this little instrument affords sufficient information: but if the true specific gravity be required with the intention of drawing conclusions from any daily alteration in the amount of solids dissolved in the urine, as for instance in a case of diabetes, when the object is to ascertain the increase or decrease in the amount of sugar present, then no reliance whatever can be placed on this instrument for any minute differences of specific weight. An accurate balance, and the thousand-grain bottle can alone be relied on. The urinometer has its index scale secured in the tube of the upper limb of the instrument with the sealing-wax. This often happens, the index gets slightly displaced, and the instrument is useless. I have known such an

instrument in use, and the displacement not discovered till repeated discrepancies between its results and those obtained by the balance led to a detection of the error. These instruments should never be washed in warm water; the finger should never be applied over the red spot where the index is secured; and they should be carefully kept in a cool place in the summer time. I have reason to think that the displacement of the index just mentioned was occasioned by the instrument being kept usually on a window-sill daily heated by the sun.

“Lehmann has shown that the formulæ given for calculating the solid constituents of the urine from the specific gravity of this fluid are inapplicable and erroneous. ‘It was supposed that the residue of the urine might readily be determined from its specific gravity, and for this purpose F. Simon, Becquerel, and G. Bird have attempted to establish formulæ from which, when the specific gravity was given, the solid residue of the urine might be determined. The complete inapplicability of such formulæ, which I have shown by my own experiments, has recently been most completely demonstrated in a large number of investigations made by Chambert on the urine of healthy persons. These experiments prove that there does not even exist any definite proportion between the quantity of salts in the urine and its density, and much less that any such connexion exists between the organic matters and the density of the fluid.’” (Lehmann’s ‘Physiological Chemistry,’ vol. ii, p. 436. Sydenham Soc. edition.)

And with respect to the proportion of albumen he adds:

“The weight of albumen contained in any single specimen of urine would convey very incorrect data on which to calculate the quantity passed in the twenty-four hours. An estimate of the quantity passed in the twenty-four hours can only be obtained by collecting all the urine passed in that interval. The ordinary method is to collect and measure the whole, and, by taking a part, the amount of albumen contained in that part represents the proportion contained in the whole. Such a process is totally inapplicable in private or general practice. It is next to impossible to collect all the urine of the twenty-four hours; what is passed during the action of the bowels cannot be collected: indeed, it is only in hospital practice that such observations can be attempted. For practical purposes of every-day use a correct and minute estimate of the amount of albumen passed is out of the question, a rude approximation is all that can be obtained, and is all that is necessary for practical purposes, and this can be obtained readily by noting the space the coagulated albumen occupies in the tube after being allowed to rest; and the phraseology recommended by Dr. Christison expressive of the proportions observed by the eye, may with advantage be employed.

“Dr. Christison’s degrees of coagulability:

- I. Gelatinous by heat.
- II. Very strongly coagulable by heat—nearly the whole tube.
- III. Strongly coagulable—half the tube.
- IV. Moderately coagulable—one quarter of the tube.
- V. Slightly coagulable—one eighth.
- VI. Feebly coagulable—less than one eighth.
- VII. Hazy by heat—no visible flakes.”

Failing to obtain reliable evidence in either of these sources, Dr. Basham

then takes the microscope and inquires whether such evidence is to be found in the changes of the tube-casts, or the cell-structures accompanying them, which appear from time to time in albuminous urine. And here he finds what he seeks for. He has found it, moreover, so often and in so many different circumstances, that all reasonable doubt must be removed as to the reality of the discovery. For several years past, indeed, he has made almost daily examinations of the urine of all the patients under his care in the Westminster Hospital suffering from renal dropsy and other forms of renal disorder, and made drawings of the most characteristic appearances in the case-books, so that a comparison of the morbid deposits of any period of the disease might be readily made, and one case easily compared with another. In this way he has found that the epithelial cells thrown off from the renal tubes, as well as the tubes accompanying them, suffer very material and obvious alteration as the disease of the kidney advances or recedes. He has found, indeed, that "attention to the microscopic character of these casts will at any time enable the practitioner to estimate the nature and intensity of the disease, its advance or decline, its form, and its probable termination. The blood casts represent the period of active hyperæmia and hæmorrhage; the coarsely granular epithelial cast, with its compound inflammation corpuscles, and accompanied by amorphous granular flakes, stained with hæmatin, the period of inflammatory exudation; the finely granular, semi-transparent casts, with scattered epithelial and granule-cells, the subsidence of that stage. Transparent casts, with compound cells, with isolated, resplendent molecules, and grape-like clusters of granules, represent a stage of chronic, sub-acute disease of grave import; and if these casts become more and more loaded with large and gradually increasing fat-granules, and even oily drops, the progress of fatal fatty degeneration is clearly marked.

It is not to be supposed that this great step in diagnosis and prognosis, for which we are indebted to Dr. Basham, is under ordinary circumstances to be made by a single examination of the urine. On the contrary, Dr. Basham guards us from expecting this, and distinctly states that it is rather by comparing the character of the deposit of one period with another, and by carefully noting the altered appearances of the casts and cells, and the tendency of the change taking place in them, that a correct estimate is to be formed of the advance or recession of the disease.

As to the rest, we would say that the lectures and cases upon which the work is based are so arranged that the characters of the simple and earlier forms of deposits are described before the characters of the more advanced stages of renal degeneration. In doing this, the general characters and symptoms, as well as the principles of treatment, are well brought out. In a work of this character plates of a very superior kind are indispensable, and these are provided with the most lavish liberality from the pencil of Dr. Basham himself, every case almost having its special drawing. We know, indeed, of no plates which surpass them in excellence, and we can say of them, without exaggeration, that the image which they convey to the eye is almost as exact as that which is transmitted by the thing itself through the microscope.

On Diabetes and its successful treatment. By JOHN M. CAMPLIN, M.D.
(London, Churchill, 12mo, pp. 60, 1858.)

This volume is a reprint of a paper published in the 'Transactions of the Royal Medical and Chirurgical Society,' together with a sequel of the author's history and experience since the publication of this essay, and a brief account of the recent physiological inquiries which relate to the subject.

The case of Dr. Camplin, we do not hesitate to say, is one of great practical importance, and bran cakes,* we feel sure, will prove to be a very valuable addition to the jувantia in diabetes.

"In November, 1844," says Dr. Camplin, "I was prostrated by an attack of diabetes. The symptoms were well marked, and need not be detailed; the professional friends consulted did not expect that I should rally; and one of them went so far as to say, when asked to prescribe for me a particular remedy, that it would only be 'smoothing my passage to the grave,' and yet here I am, with urine varying from 1·016 to 1·020, instead of 1·040 and upwards, and although ten years older, seldom or never flinching from any professional duty.

"There being no doubt as to the nature of my complaint, an altered diet was immediately advised, and a run to the Isle of Wight; the latter I could take for a few days only, and that in the commencement of a cold, dreary winter, yet it assisted materially in arresting my downhill course, and time was gained for the effect of remedies and diet.

"All my advisers (and I had several, whose kindness can never be

■ *Formula for Bran Cakes.*—Take a sufficient quantity (say a quart) of wheat bran, boil it in two successive waters for a quarter of an hour, each time straining it through a sieve, then wash it well with cold water (on the sieve), until the water runs off perfectly clear; squeeze the bran in a cloth as dry as you can, then spread it thinly on a dish, and place it in a slow oven; if put in at night let it remain until the morning, when, if perfectly dry and crisp, it will be fit for grinding. The bran thus prepared must be ground in a fine mill and sifted through a wire sieve of such fineness as to require the use of a brush to pass it through; if grinding once is not sufficient, it must be ground again until quite soft and fine. Take of this bran powder 3 oz., three new-laid eggs, 1½ (or 2 oz. if desired) of butter, and rather more than half a pint of milk, mix the eggs with a little of the milk, and warm the butter with the other portion; then stir the whole well together, adding a little nutmeg and ginger, or any other agreeable spice. Bake in small tins (pattipans), which must be well buttered, in a rather quick oven for about half an hour. The cakes, when baked, should be as thick or a little thicker than a captain's biscuit; they may be eaten with meat or cheese for breakfast, dinner, and supper; at tea they require rather a free allowance of butter, or may be eaten with curds or any of the soft cheeses.

"In the 'Medical Gazette,' May 2d, 1857, was published what I considered an improved form, which resembled the above in its composition, only that I directed, first, 35 grs. of sesqui-carbonate of soda, and then 3 drachms of dilute hydrochloric acid, to be stirred into the mass immediately before baking, and that it should be baked in a bason in one loaf or cake.

"This amended form, as it was termed, is very agreeable, but requires more management, and does not keep so well; consequently, I now generally recommend the old formula.

"I cannot be too particular in directing that the bran should be reduced to a very fine powder, and, in order to this, as well as to deprive it of its starch, it should be well washed and dried; by this means it is rendered friable, and the process shortened—that which remains after the first sifting, should be ground again and again until it becomes fine."

repaid) recommended meat, fish, and eggs, with the cruciferæ; they differed, however, in minor points; one advised coffee, another tea; one wine, another brandy, &c. As a substitute for bread, cakes or biscuits made of washed flour and lard were at first recommended; these soon quite disagreed. The gluten bread was next tried, this latter, unpleasant as it was, I took as long as it could be borne; but, after a year or two, it became insupportable, and when passing a few days at Brighton I availed myself of the advantages of change of air, and Sussex bread, to cast it aside, and have never resumed it. On returning to town, I did pretty well for a time, eating the ordinary bread very sparingly, but afterwards I relapsed, and was almost in despair. Having before this seen Dr. Prout occasionally, and now telling him of my dilemma, he suggested a kind of bran cake, which was immediately prepared according to his directions: it was by no means a pleasant composition, but that was not the worst, for the bran acted powerfully on the bowels, and it could not be continued in that form. We have all heard that 'necessity is the mother of invention,' and I immediately set a mill-maker to work to make me a mill which should grind the bran into a very fine powder; this means, and careful sifting, overcame the difficulty, and enabled me to succeed in the preparation of a kind of cake which was continued for some years, and with the best effects.

"I had before this been more rigid in my diet than directed by the doctor, who, to use his own expression, 'tolerated things which he did not advise.' My protracted sufferings, however, now determined me to put away everything saccharine or amylaceous to the utmost possible extent, and I therefore gave up wine, at the same time that the bran cake enabled me to discard entirely the use of bread. I now soon became decidedly convalescent, and have never had my diabetic symptoms return with violence. The cold of November still produces unpleasant feelings, and some anxiety, but has not for two or three years rendered necessary even a partial return to the bran cake; and for six or seven winters past I have not required the double clothing, and the hot bottle in the carriage, which were sometimes indispensable. There can be no question as to this bran cake having greatly prolonged my life, as I was fast becoming a decrepit valetudinarian before its use, and should, in all probability, have altogether broken down long since; and I am the more anxious to dwell on this part of my history, from the conviction that if the bran cake, or some modification of it, were made on a larger scale, it might be used in our hospitals, and patients might be supplied with it after their discharge, thus obviating one of the difficulties as to their diet, after temporary recovery.

"At the present time I feel well, so far as the diabetes is concerned, yet am of opinion that it would be no difficult matter to bring on a return; and believe that there are few who recover so perfectly as to be able to use the indiscriminate diet of former days. I am indeed acquainted with some who go so far as to take potatoes, and even sugar and fruit; but they do this very sparingly, whilst others have fallen speedy victims to imprudence in this respect; and I would never recommend any one to make the experiment without great caution.* In

* No experiment on diet should be made in the winter, particularly if the north-east wind prevail.

my own person, the specific gravity has increased from 1·025 to 1·037, from taking a glass or two of fruity port wine, without any other assignable cause; and rice puddings, persisted in for a short time, would formerly produce the same effect. I say formerly, with regard to rice, for during the late epidemic I suffered rather severely from choleraic diarrhœa, and scarcely know what would have been my fate if restricted to my old diet of meat, greens, and bran cake: as it was, rice and macaroni many days together (without any fresh vegetables) produced no alteration of urine, though a sweet taste after meals sometimes reminded me of former days.

“To return to the early history of my case.

“Fat meat and eggs were more especially directed for me, and were taken without any immediate ill effects, but I am of opinion that they produced great biliary derangement, more especially the eggs, the free use of which has been laid aside long since.

“Fish is a most important article of diet for the diabetic, but does not require particular notice, as its use only requires the ordinary cautions for those in moderate health.

“I have never found it necessary to disallow the use of milk; the sugar contained in it certainly does not pass into glucose readily, or under ordinary circumstances; and this induces me to notice, that, as to farinaceous substances, their disposition to pass into sugar is not to be estimated simply by their proportions of gluten and starch, and it does not seem to me that anything but experiment will determine the degree in which they do this. Some kinds of bread injure more than others; and in my own person brown bread has often produced greater sweetness in the saliva than that made of fine flour; and it is my opinion that unfermented flour in the various forms in which we use it is less liable to pass into sugar than bread. I have not, however, had opportunity to test this on a sufficiently large scale to be certain of the fact. Genoa macaroni I believe to be one of the best substitutes for the bran cake; the finest macaroni not only contains more gluten than ordinary bread, but the long boiling in water which it ought to undergo before being prepared for the table, further diminishes the proportion of starch.

“With regard to vegetables, I have almost confined myself to the cruciferæ, as they can be obtained in London during the greater part of the year; the young cabbage, is, perhaps, at once the cheapest and best for ordinary use. Cauliflowers, broccoli, Brussels sprouts, &c., give considerable variety. Seakail is excellent, but rather too expensive; the late Dr. Pereira recommended sour kroust to me; but having fresh vegetables at hand I have never tried it. Since my recovery, I have taken French and scarlet beans, without injury; but should consider the cruciferæ highly preferable for the actually diabetic. Spinach is generally considered allowable, and indeed recommended; and those who reside in the country might add to the list other plants of the same natural order, such as the *Chenopodium bonus Henricus*, and the younger leaves of the common beet, both of which I have tasted, and found them very agreeable. My friend, Mr. Ward, in conversation with him on the subject, suggested that the leaves of the *Beta Cicla* and *Maritima* may be used as pot-herbs; and perhaps the list might be still further enlarged.*

■ Another of the cruciferæ, the watercress, will help to make the breakfast agreeable,

"As to tea or coffee, I have no hesitation in giving the opinion that, in a majority of cases, tea is to be preferred. Milk may be taken with it freely; cream sparingly.

"As a beverage at meals, water or toast-water may be used, and, instead of wine or malt liquor, a small quantity of brandy and water, not above a table-spoonful of the former. Wine is better excluded, except claret, which is too expensive for common use. There are several other wines which may be allowable as being free from sugar, but of them I have no experience. The pale French brandy is, no doubt, the best; but I have tried the English eau de vie made in imitation of it, and found it to answer very well, and Dr. Bence Jones informs me that he has sometimes directed rum, which, being without sugar, is, *quoad hoc*, as eligible as brandy. Sponging with tepid water, followed by friction, has been so beneficial, in more than one case in which I have been consulted, as to call forth the highest encomiums. For myself, I have only practised sponging with cold salt and water in the summer, and an occasional warm bath in the winter: these I have used with great advantage.*

"Warm clothing—a leather waistcoat, and gutta percha soles to the boots, in winter, are very important. It would be superfluous to descant on the advantages of change of air and occupation; but I may mention that, whilst at home, I constantly used the bran cake; if I left town for a time, and took the prepared bran with me, I seldom used it beyond two or three days, and never felt the worse for taking the liberty of substituting bread under the influence of change of air and scene. When I returned home I continued the bread, until warned by a partial recurrence of symptoms to have recourse to my bran cake. This took place several times before I was able altogether to discontinue its use."

Dr. Camplin does not go into the *medical* treatment of diabetes. He does not know, he tells us, of any specific remedy. Bitters and alkalies did him great service at one period of his attack, and the remedy which he is disposed to mention before others is the citrate of ammonia in the effervescent form, generally combined with citrate of iron. Speaking of the cause of his malady, the author says:

"In my own case, two causes had long been at work: the wear and tear of a laborious profession, and a diet in which fruits, rice, &c., had too large a share: and the disease, which had no doubt been creeping on unobserved for some time, was brought to its acme by eating freely of apples, which the unnatural thirst rendered peculiarly grateful, and at the same time undergoing unusual fatigue, and this too in the month of November. During one night passed in watching a somewhat anxious case, the urine became very great in quantity, as well as abnormal in appearance, and the nature of the complaint was too obvious. My pallid cheeks and sunken countenance, then and for a long time afterwards, will not soon be forgotten by myself or my friends."

Notwithstanding a pretty free use of food containing starch there was

and I see no objection to other vegetable products, such as celery, endive, mushrooms, &c., when in season, if the stomach will bear them, provided we avoid starch, sugar, and the vegetable acids.

* I think it is important that the bathing should be followed by friction, and, if plain water is used, hair gloves should be employed, or some similar means of encouraging a sustained action on the skin.

no return of the symptoms of diabetes until November, 1854, when the setting in of the cold winds appeared to be the cause.

"When these symptoms had attained considerable severity, they were partially checked by additional warm clothing, and restriction to a very small quantity of brown bread; at length this failed, and on the 5th of March the specific gravity of the morning urine being 1·041, that of the afternoon 1·035, the quantity very considerable, with dry mouth, and tenderness in the back, particularly over the right kidney, it began to be time to take some decisive step; and I resumed the bran cake. The effect was immediate. On the 8th, the morning urine was scarcely 1·020, the afternoon 1·015, the quantity normal; and although taking no bread, and less meat, I was already beginning to regain flesh;* that the change was owing to the substitution of the cake for the brown bread was sufficiently obvious, as the weather continued cold, with a prevalence of north-east wind, and I had still considerable remains of catarrh.

"Since the weather has become mild, I have been able to resume amylaceous food partially, and without injury; and, in fact, consider myself well.† The cessation of the diabetic symptoms was followed by congestion in the head, which harassed me several days, but gradually subsided under the use of citrate of ammonia, and small doses of Vin. Colchici; the uneasiness in the back left me at once, as well as several sensations difficult to be described. I have not yet ventured to return to the use of bread, but continue the bran cake at breakfast and supper; at dinner, two or three times a week, after meat, or fish, and greens, I allow myself puddings of flour or macaroni; and at tea, small cakes made with flour, &c."

Another relapse took place in the autumn of last year during a few days' relaxation at the sea-side.

"Accidental exposure to the east wind and damp produced severe fever and rheumatism, which first confined me to bed, and then sent me home very much broken down. After some time these complaints were subdued, but great debility remained, and I perceived a return of old symptoms, in the frequency of micturition, and, on examining the specific gravity of the urine found it 1·036.

"For a long time previously I had taken bread regularly, and other farinaceous matters occasionally; but, with this state of things, determined to return to the bran cake.

"On September 24th, my urine, according to my friend Dr. Garrod's glucometer, contained seventeen grains of sugar to the ounce.

"September 28th (only four days after the alteration of diet), it was found to contain less than half a grain, indeed only a trace.

"For a short time I followed my plan rigidly, restricting myself to meat or fish, greens, and the bran cake—the result was a very speedy return of health and activity; since then my diet has been that of convalescents."

Dr. Camplin considers that fourteen years of health and comfort have

* I have often remarked the rapidity with which this has taken place; it has been much more striking than its opposite.

† In addition to my own observation (with both the potash and copper tests), I may mention that my friend Dr. Bence Jones kindly examined a specimen the other day (June 19th), the sp. gr. was 1·020 and he found no trace of sugar.

been added to his own life by this mode of treatment. Nay, he considers that his life has been actually prolonged by the diabetes, and we would direct especial attention to this remark. It is quite a new view of the matter, and it is one also in which we are very much disposed to concur.

"However paradoxical it may appear," says our author, "I believe that my life has been prolonged by the attack of diabetes—previously I was dyspeptic and bilious, and had symptoms which, there is no doubt in my own mind, would have proceeded to fatty degeneration of the heart—the meat diet first, and then its combination with the bran cake, altered this state of things, and I have now firm muscles, and am a stranger to the feeling of want of power in the circulation.

"Since that time, I have been more anxious to direct patients, at all disposed to obesity, with a feeble circulation, to take animal food (with fruit and vegetables), rather than bread, rice, and potatoes."

We must add, also, that the same favorable results have been witnessed in several cases which have been treated by the author after this method.

The account of the recent physiological inquiries which relate to diabetes show that the author is not behindhand in these matters. Under this head, Dr. Pavy's recent investigations are particularly referred to. Dr. Camplin admits the importance of these researches into the glucogenic properties of the liver; but his own sensations and reasonings, he tells us, cause him, after all, not to discard the views of Prout respecting the pathology of diabetes, and to refer the formation of sugar solely, and in all cases, to the liver. And on this point we are altogether of the same opinion. Indeed, we shall not be surprised if, before long, we have to leave Bernard, and go back to Prout for the pathology of diabetes. At any rate this would seem to be a possible consequence of the recent investigation of M. Sanson.

In conclusion, we would cordially recommend the work before us, as a really important contribution to practical medicine.

The true Physiological Method of restoring Persons apparently Drowned or Dead, and of resuscitating Still-born Children. By HENRY P. SILVESTER, B.A., M.D. Lond. (Pamphlet, Churchill, 1858.)

The *new method* which Dr. Silvester brings before the profession is a simple imitation of natural deep respiration, and is effected by means of the same muscles as are employed by nature in that process.—In ordinary deep inspiration we lift the ribs and sternum by the pectoral and other muscles which pass between the chest and the shoulders, and thus produce the threatened vacuum which inflates the lungs. In the new method we lift the ribs and sternum by the pectoral and other muscles, which pass from the shoulders to the parietes of the thorax, by steadily extending the arms up by the side of the patient's head; by elevating the ribs the cavity of the chest is enlarged, a tendency to a vacuum is produced, and a rush of air immediately takes place into the lungs. Expiration is brought about by simple compression of the sides of the chest by the patient's arms.

The principle.—Forced enlargement of the capacity of the chest,

producing a tendency to a vacuum, and consequently an inspiration of air into the lungs, induced by the constrained action of the muscles of ordinary and extraordinary inspiration upon the moveable walls of the thorax.

Diminution of the capacity of the chest and expulsion of the air from the lungs, and consequently an expiration, induced by compression.

The arms of the patient are to be used by the operator as handles to open and close the chest.

This new method has been tested by experiment on the dead body by the usual apparatus, the elastic tube being securely fastened to the trachea.

The body was placed on its back, supported, and a little raised by a small pillow placed under the shoulders. The height of the column of fluid having been first carefully noted, the arms of the subject were raised, and steadily extended upwards by the sides of the head, so as to draw up the shoulders and put the pectorals on the stretch, elevate the ribs, and consequently enlarge the cavity of the chest. The result was that the fluid in the bent tube rapidly fell, and so considerably as to recede high up in the leg of the instrument nearest the body, that is to say, the tendency to a vacuum produced in the chest drew the air into the lungs. The shoulders and arms were next pressed down upon the sides of the chest, and immediately the fluid rose as much above its usual level in the further leg of the apparatus as it did in the foregoing experiments; demonstrating:

1. That the actual capacity of the chest was increased, and air drawn into the lungs by the constrained action of the muscles of respiration upon the moveable walls of the thorax.

2. That expiration was produced by pressing the arms and shoulders down upon the sides of the chest.

The distinguishing feature of this method is the actual enlargement of the cavity of the chest—the elevation of the ribs above their ordinary or natural level.

It is, of course, of consequence to get as much air into the lungs as possible, because there can be but little doubt that fresh air is the proper stimulant to the respiratory efforts, just in the same way that light is to the eye and sound to the ear, but acting more entirely by reflex action. The quantity of air respired, according to Dr. Silvester's experiments on the dead body, appears to be about ten times greater in the postural method than in the method of Dr. M. Hall.

The mechanism of respiration in man.—The general principle of the operation is this: The lungs are divided into cavities of extreme minuteness; and these cavities or air-cells are all connected with the trachea by means of the bronchial tubes. The lungs themselves are suspended in a cavity that is completely closed, being bounded above and around by the bony framework of the thorax, the interspaces of which are filled up by muscles and membranes, and being entirely cut off from the abdomen below by the diaphragm. Under ordinary circumstances, the lungs completely fill the cavity. But the capacity of the thoracic cavity is susceptible of being greatly altered by the movements of the ribs, and by the action of the diaphragm and abdominal muscles. When it is diminished, the lungs are compressed, and a portion of the air contained in them is expelled through the trachea. On the other hand, when it is

increased, the elasticity of the air within the lungs causes them immediately to dilate so as to fill the vacuum that would otherwise exist in the thoracic cavity, and a rush of air takes place down the air-tubes and into the remotest air-cells, to equalise the density of the air they include (which has been rarified by the dilatation of the containing cavities) with that of the surrounding atmosphere. The lungs themselves appear to be almost entirely passive instruments of the respiratory function. The dilatation of the cavity of the chest, which constitutes inspiration, is accomplished by two sets of movements—the elevation of the ribs, and the depression of the diaphragm. In tranquil breathing, the contraction of the diaphragm is alone nearly sufficient to produce the necessary enlargement of the thoracic cavity, the position of the ribs being very little altered. In the act of deep inspiration, the ribs (whose ordinary direction is forwards, sloping downwards), under the influence of their elevator muscles—namely, the pectoralis muscles, major and minor, the serratus magnus, the scaleni muscles, and the intercostals—pass from the sloping to the horizontal position. By this change the dimensions of the chest are enlarged in the transverse as well as in the antero-posterior direction; for the middle curved portions of the ribs are carried outwards, and therefore brought further apart from each other, and their sternal extremities are moved forwards, accompanied by the sternum, the distance of which from the dorsal vertebræ is thereby increased. When the respiratory movement is very forcibly performed, the scapula is itself drawn upwards, thus producing an increased elevation of the ribs, and an unusual enlargement of the upper part of the thoracic cavity. When deep expiratory action is to be performed, the ribs descend by the action of the muscles of the spine and the abdomen, the diaphragm being altogether passive. In this manner, by the regularly alternating dilatation and constriction of the thoracic cavity, the air within the lungs is alternately increased and diminished in amount; and thus a regular exchange is secured. The number of the respiratory movements (that is, of the acts of inspiration and expiration taken together) may be estimated at from fourteen to eighteen per minute.

It may also be remarked, that from the peculiar mode in which the ribs are articulated with the spinal column at one extremity, and from the angle which they make with the cartilages that connect them to the sternum at the other, the act of elevation tends to bring the ribs and their cartilages more into a straight line, and to carry the former to a greater distance from the median plane of the body, whilst the sternum is also thrown forwards; consequently the elevation of the ribs increases the capacity of the thorax upwards, forwards, and laterally. Although the range of motion between each vertebra and the ribs attached to it is very limited, yet the whole framework of the chest enjoys such mobility, that by a deep inspiration its cavity is sometimes more than doubled.

In difficult respiration, moreover, the muscles of the limbs are made to assist in respiration—the patient seizing hold of any fixed object for the sake of a firm point for the muscles to act from; and that in deep respiration the greatest enlargement of the thoracic cavity in both sexes is made by the ribs, and not by the diaphragm. It appears very questionable whether the diaphragm is affected any further than being flattened, and that without descending.

The following rules for the treatment of apnoea are deduced from actual experiment, and are in accordance with established physiological principles.

I. *To adjust the patient's position.*

Place the patient on his back, with his shoulders raised and supported on a folded article of dress.

II. *To maintain a free entrance of air into the windpipe.*

Draw forward the patient's tongue, and keep it projecting beyond the lips. If the lower jaw be gently raised, the teeth may be made to hold the tongue in the required position. Should it be found necessary, the tongue may be so retained by passing a handkerchief under the chin and fastening it over the head.

III. *To imitate the movements of deep respiration.*

Raise the patient's arms upwards by the sides of his head, and then extend them gently and steadily upwards and forwards for a few moments (This action enlarges the capacity of the chest by elevating the ribs, and induces inspiration.)

Next, turn down the patient's arms, and press them gently and firmly for a few moments against the sides of the chest. (This action diminishes the cavity of the thorax, and produces a forcible expiration.)

Repeat these measures alternately, deliberately, and perseveringly, fifteen times in a minute.

IV. *To induce circulation and warmth, and to excite inspiration.*

Rub the limbs from the extremities towards the heart. Replace wet clothing by warm and dry covering. Occasionally dash cold water in the patient's face. These measures are perfectly compatible with the systematic performance of the imitation of the movements of respiration. A similar remark applies to the use of the warm-bath, if indicated.

Explanatory remarks.

Rule I.—This posture is not essential; but in the position recommended the vital capacity of the chest is larger than in any other recumbent attitude. The bony framework of the chest is more free to move, and both sides can be expanded at the same time. This, in fact, is precisely the posture chosen by persons suffering from dyspnoea.

Rule II.—In this way the patulous orifice of the windpipe is raised and drawn forward, so that nothing intervenes between it and the natural channel of air through the nose. The tongue is entirely prevented from falling back into the throat, whilst the extent to which the windpipe is put on the stretch is clearly indicated. The pharynx also is sufficiently opened to allow of the removal of liquids, &c., from the mouth, nose, pharynx, &c., if those have not been completely displaced by previous suitable treatment.

Rule III.—This process, in short, accomplishes artificially for the patient exactly what he himself would effect, and by the same muscles, if he had but the will and the power to draw a deep inspiration.

Possibly the elevated position of the arms, together with the muscular compression exerted on the veins of the upper extremities, might favour the descent of blood from them into the chest, at the same time that the

tendency to a vacuum produced in the thorax by the elevation of the ribs was inducing a rush of fresh air into the lungs.

At the same time that the arms are extended steadily upwards, the lungs might be filled with air by a mouth-to-mouth inflation.

The following are some of the advantages of this new method :

1. Inspiration may be made to precede expiration, or it may be second in order, at the will of the operator.
2. The expansion of the thorax is artificially ensured, and wholly under the control of the operator.
3. It may be adopted when the patient is in the warm-bath.
4. The patient is not liable to be injured by the manipulation.
5. The contents of the stomach are not liable to pass into the windpipe.
6. The tongue is prevented from obstructing inspiration.
7. Both sides of the chest may be equally inflated.
8. This process is entirely in harmony with that of nature.
9. A larger amount of air is inspired than by any other method.
10. This method is most easy of adoption.
11. Pure atmospheric air is inspired.
12. No apparatus is required.

The Royal Humane Society directs its attention mainly to the circulation ; Dr. Marshall Hall principally to the respiration.

It is intended by this method to combine the advantages of both these plans.

A successful case of resuscitation has already been recorded.

A Dictionary of Medical Science, containing a concise explanation of the various subjects and terms of anatomy, physiology, pathology, hygiene, therapeutics, pharmacology, pharmacy, surgery, obstetrics, medical jurisprudence, dentistry, notices of climate and of mineral waters, formulæ for officinal, empirical, and dietetical preparations, &c., with French and other synonyms. By ROBLEY DUNGLISON, M.D., LL.D., Professor of the Institutes of Medicine in the Jeff. Med. Coll., Philadelphia. Fifteenth edition, revised and very greatly enlarged. (London, Trübner and Co., imp. 8vo., pp. 992, 1858.)

On the present edition of this well-known and invaluable work the author has bestowed more than usual labour. He has "revised and corrected" the whole, and besides this he has added about *six thousand subjects and terms*. The publisher, also, has done his part of the work in a very creditable manner, and we have as little to desire in the manner in which it is got up, as in the matter itself.

The Microscope in its application to practical medicine. By LIONEL BEALE, M.B., F.R.S., Physician to King's College Hospital, Professor of Physiology in King's College, &c. Second edition, with 270 woodcuts and a coloured plate. (London, Churchill, 8vo, pp. 390, 1858.)

The author has greatly increased the usefulness of this very valuable work by revising it throughout, and by rewriting several of the articles.

He has also omitted much that related merely to manipulation in the first edition (which will be found in his 'How to work with the Microscope'), and added instead much that bears exclusively upon medicine, with sixty new and original woodcuts. He has, indeed, produced a work which is thoroughly practical and useful.

On Cough; its causes, varieties, and treatment, with some practical remarks on the use of the Stethoscope as an aid to diagnosis. By ROBERT HUNTER SEMPLE, M.D., Physician to the Northern Dispensary. (8vo, Churchill, 1858, pp. 174.)

This book, which consists of a sound and practical exposition of the subject of which it treats, is divided into three parts: in the first, the anatomy of the parts concerned in cough is briefly described; in the second, the physiology of that affection is explained; and in the third, the pathological condition of the organs and tissues in the diseases characterised by cough, is successively detailed, and the treatment of each case is deduced from the morbid manifestations which give rise to it.

In the pathological and therapeutical portion of the work, the diseases of the respiratory organs occupy, of course, the most prominent place. On the much vexed question of the treatment of pneumonia we find the following remarks:

"The treatment of pneumonia has undergone a great number of changes during late years; it was treated by Laennec, with great success, by the administration of large doses of tartar-emetic; some twenty years ago it was the custom to bleed largely in all the cases which would bear it; but in the present day bleeding is not so often practised, and some are even disposed to think that antiphlogistic treatment is unavailing or injurious. My own opinion is that the type of pneumonia, like that of many other diseases, has changed of late years, and that patients will not bear bleeding so well as they did at former periods. During my pupilage I bled a great number of persons, under the direction of my medical instructors, and I afterwards did so upon my own responsibility in a large public practice, and I cannot call to mind any cases where the results were not satisfactory. Of course, it may be said that the patients recovered in spite of the treatment, and not in consequence of it; but I cannot come to that conclusion when I recollect the immediate and marked benefit which resulted from the adoption of this measure, and the perfect recoveries which generally ensued. If the same mode of treatment is not so successful at the present day, I believe the cause to be, that disease has now a tendency to run rather into the low and depressing forms, than into what may be called the high or sthenic forms; and that, therefore, the treatment, generally speaking, ought, in the present day, to be rather of an alterative or supporting character. I never was more struck with the difference existing in the treatment of disease, and, as I believe, in its type, than when I had occasion for some months to attend the practice of the London Fever Hospital. In that institution, which was at first (I speak of some twenty or thirty years ago) situated in a low, crowded, and

unhealthy neighbourhood, the patients were generally bled largely, and were otherwise subjected to lowering treatment, and the results were quite satisfactory, as was proved by the frequent recoveries which took place; but at the present period, when the patients are situated in an almost palatial residence, isolated, clean, lofty and well-ventilated, they will not bear depletion, and they are actually never bled at all, and even a leech is hardly ever used in the institution. The present accomplished and experienced physicians of the establishment, Dr. Tweedie and Dr. Southwood Smith, have been in the habit of treating fever and its complications for a great number of years, both in the old and the new buildings, and I do not believe that their former plan of bleeding, and otherwise lowering the patients, is in any way inconsistent with their present system of strengthening and supporting them, but that the recent epidemic constitution requires a different mode of treatment to that which was formerly required."

After describing the different treatment required in the various forms and complications of pneumonia, the subject is thus summed up:

"Thus it will be seen that the treatment of pneumonia is very different, according to its nature, the subjects whom it attacks, and its complications with other diseases; and this diversity of treatment may be explained upon rational principles. Those who contend dogmatically that pneumonia should be treated always with depletion, and those who maintain that it should be combated by stimulants, are both right and both wrong; the true method of medication consists in a careful study of all the features of the case, and the adoption of measures which are calculated to be serviceable in the peculiar conditions which the malady may happen to present."

In reference to the treatment of pulmonary consumption, the following remarks are made upon the employment of cod-liver oil:

"Viewing pulmonary consumption as a variety of scrofulous disease, and observing the very marked influence exerted over such affections by cod-liver oil, it can hardly be doubted by any practitioner that there is some solid advantage in using this agent. Whether it has really the power of arresting the progress of pulmonary tubercle, or causing its absorption, can never be determined with certainty, because we have no means, except by the ear, of ascertaining the condition of the lungs in the living state; but that it does possess such a power is extremely probable, from the results of observation in phthisis and in allied diseases. I have been very much struck by witnessing its efficacy in the scrofulous diseases of children, in whom its operation appears almost marvellous, and in whom, under its use, the most decided symptoms of scrofulous disease unquestionably disappear. Those who admit the latter class of facts, attribute its efficacy to its employment as a dietetic agent; but surely this is begging the question, for if a remedy proves beneficial, of what importance is it whether it is regarded as a medicine or an article of diet? But if it is meant that other articles of diet are equally efficacious, and that if the children of the poorer classes were better fed they would not require cod-liver oil, I think that

the reasoning is not consistent with the facts; for it must be remembered that scrofulous disease is not confined to the poor, but that it attacks the children of the wealthier and better classes, who have abundance of food and good nursing; yet in such cases the effects of the oil are quite as striking as among dispensary and pauper patients."

The following observations are made upon the subject of the expatriation of consumptive patients:

"I cannot refrain from expressing an opinion upon a point on which much discussion has taken place, namely, the propriety of sending away a consumptive patient to a distant land, in the hope of curing the disease in his lungs. I must own my conviction that not only very little good, but very much harm is generally done by such a proceeding. A patient is too often torn away from his home and his relatives, to perish in a foreign soil; or after a brief sojourn in the land of his banishment, to return in a worse condition than when he went away. When the patients are fond of travelling, are able to bear the expense of it, and can carry their relatives and their household with them, there may be an advantage in a trip to the Mediterranean, or a residence in the south of France, or a tour in Egypt; but in the great majority of cases, our own country affords as much physical benefit to the sufferer, and is perhaps far more congenial to his sentiments and his affections, not to mention pecuniary and other domestic considerations."

In the treatment of acute laryngitis the antiphlogistic plan is insisted upon, and the opposite system is thus denounced:

"While treating of this subject, I cannot help denouncing with all the force which I possess, the inefficiency, and I might add the knavery of the so-called homœopathic system, in the management of such a disease as acute laryngitis, in which it may be said '*horæ momento, aut cita mors venit, aut victoria lætæ*,' and in which the delay of efficient measures may entail the worst results. To mock the sufferings of a dying patient, by the administration of inert and useless globules, while the adoption of a rational and vigorous plan of treatment might restore him to life, appears to me to be nothing less than to ridicule human misery, and to welcome the approach of the angel of death. I can only hope that if any honest homœopath (if there be such a person) should meet with a case of acute laryngitis, he would, at least for the occasion, renounce his creed, and prefer the sacrifice of a dogma to the destruction of a fellow-creature. Nor can I omit to observe, that I by no means coincide in the views of those who believe that because diseases have, in the present day, changed their type, and because bleeding is not generally so well borne as it formerly was, *therefore*, all bleeding and all depletion are injurious. I believe, on the contrary, that in certain cases the abstraction of blood is not only justifiable but is imperatively demanded, and I believe that acute laryngitis is a case in point."

II.

REPORT ON THE PROGRESS OF SURGERY.

On Amputation by a long and a short Rectangular Flap. By THOMAS P. TEALE, F.R.C.S., Surgeon to the Leeds General Infirmary, and Member of the Medical Council of Great Britain. Illustrated by engravings on wood. (London, Churchill, 8vo, pp. 72, 1858.)

THE mode of amputation described in this work will, if we mistake not, be adopted by all surgeons as soon as they become acquainted with it, and this not only because it produces a more perfect and useful stump than that which can be obtained by the ordinary circular and flap operations, but because it lessens appreciably the attendant risks of death. As the title sets forth, Mr. Teale proposes that amputation should be performed by a long and a short rectangular flap—the long flap, which folds over the end of the bone, being formed of parts which are generally devoid of large blood-vessels and nerves.

“The size of the long flap is determined by the circumference of the limb at the place of amputation, its length and its breadth being each equal to half the circumference. The long flap is therefore a perfect square, and is long enough to fall easily over the end of the bone. In selecting the structures for its formation, such parts must be taken as do not contain the larger blood-vessels and nerves. A flap so formed will be for the most part anterior in position as far as regards the general aspect of the body, but superior when the patient is in the recumbent posture, as during the after-treatment.

“The short flap, containing the chief vessels and nerves, is in length one fourth of the other.

“The flaps being formed, the bone sawn, and the arteries tied, the long flap is folded over the end of the bone; each of its free angles is then fixed by suture to the corresponding free angle of the short flap. One or two more sutures complete the transverse line of union of the flaps. At each side the short flap is united to the corresponding portion of the long one by a point of suture, and one suture more unites the reflected portion of the long flap to its unreflected portion. Thus the transverse line of union is bounded at each end by a short lateral line at right angles to it.

"After the patient has been carried to bed, the stump is laid on a pillow, over which a large sheet of gutta-percha tissue has been spread. *No dressing whatever* is required in the early part of the treatment. A light piece of linen or gauze is thrown loosely over the stump and pillow, and these are protected from the pressure of the bedclothes by a wire-work guard. To relieve tension the lateral sutures may be removed on the following day, but those of the transverse line may be allowed to remain until they are cast off, or appear no longer needed on account of the consolidated union of the parts. When the sutures of the transverse line have lost their hold, if the flaps should gape, a strap or two of adhesive plaster may be applied. Simplicity in the treatment is thus secured, as well as disturbance of the stump avoided.

"To carry out these objects completely, the attendants and nurses must be strictly enjoined not to lift the stump from the pillow without the authority of the surgeon. As there are no dressings to be soiled, and therefore to require removal, the stump generally need not be raised from the pillow for many days, or even for two or three weeks. When there is a discharge of matter, the nurse must remove it frequently by a soft sponge from the subjacent gutta-percha, without lifting the stump.

"The chief advantages of this mode of operating are—

"1st. The avoidance of tension.

"2dly. The formation of a soft covering for the end of the bone consisting of parts free from large nerves.

"3dly. The non-disturbance of the plastic process, and the consequent placing of the large veins of the limb, as well as the smaller veins of the bone, in a condition the least likely to take up purulent matter and putrid blood or serosity.

"4thly. The favorable position of the incisions for allowing a free outlet for purulent and other discharges.

"The avoidance of tension is secured by the ample size of the long flap. For although the tonic contraction of the divided muscles is allowed to go on unrestrained by circular bandaging or adhesive dressings, the flap is still amply sufficient to cover the end of the bone; indeed, at the time of the operation it often appears superabundant, but in the result it is not found to be so, chiefly in consequence of the great retraction of the short flap.

"The non-disturbance of the plastic process is the chief point on which the future safety of the patient depends. The long flap folding over the end of the bone, and being free from tension, soon acquires an organic union with it. The open mouths of the veins of the bone are thus early sealed; and the chief veins of the limb, protected in the retracted short flap, and undisturbed by unnecessary liftings and dressing of the stump, have also the best opportunity of becoming permanently closed, and of being thereby rendered incapable of taking up purulent and putrid matters.

"There are, however, causes unfortunately beyond our control, which frequently oppose the plastic process. These are epidemic influence, hospital air, the peculiar condition of the general atmosphere, and, more serious than all, the effects of *shock*. The evils of shock are not only immediate, but also remote. A person in robust health may, by the immediate effect of shock from injury, have his life nearly extinguished,

and may so far rally as to be submitted to amputation, but the *remote* effects of the shock are still in store for him. In such a case the vital condition of the blood and of the whole fabric of the body may remain so far lowered as to be incapable of setting up a *vigorous* process of repair.

"How careful, therefore, ought we be to husband to the utmost the feeble reparative power that exists."

Mr. Teale then proceeds to set forth the results of this operation during a period of three years, extending from June 16th, 1855, to June, 16th 1858. The operation, he tells us, has been performed 56 times, 50 times in the Leeds Infirmary, by Mr. Teale and his two colleagues, Mr. Smith and Mr. S. Hey, and 6 times in private practice, by Mr. Teale himself, his son, and Mr. Wheelhouse. The particulars of the cases are given; the results are as follows:

56 Amputations.	Thigh . . . 18	{	Accident . . . 1	{	Death 0
			Disease . . . 17		Recovery . . . 1
	Leg 28	{	Accident . . . 1	{	Deaths 3
			Disease . . . 27		Recoveries . . 14
	Arm 6	{	Accident . . . 3	{	Death 0
			Disease . . . 3		Recovery . . . 1
	Forearm . . . 4	{	Accident . . . 1	{	Deaths 1
			Disease . . . 3		Recovery . . . 2
		{	Accident . . . 1	{	Death 0
			Disease . . . 3		Recovery . . . 1
	{	Accident . . . 1	{	Death 0	
		Disease . . . 3		Recoveries . . 3	

"To determine the value of this mode of operating in lessening the mortality of amputation, the facts hitherto at our command are manifestly insufficient; but, as far as they extend, their bearing upon this point is highly favorable. The results are more important, as they extend over so considerable a period of time as three years, and as being drawn from the practice, not of one surgeon, but of several.

"The traumatic cases in this series form but a small proportion of the whole—namely, 6 in 56. It may, however, be remarked that of these 6 cases only 1 died; and that the 2 cases of amputation of the lower limb for accident both recovered. Whereas of 17 traumatic amputations performed during the same period of time in the Leeds Infirmary, by the same surgeons, 10 died, the deaths being in the proportion of 1 in $1\frac{2}{3}$ cases.

"It is, however, only in the amputations of the thigh and leg for disease, as shown in the preceding summary, that the numbers are large enough to warrant their comparison with the standard obtained at the London and provincial hospitals.

"The amputations of the thigh for disease present 3 deaths in 17 cases, or nearly in the proportion of 1 in 6. In the London hospitals the ordinary amputations of the thigh for disease show a mortality of 1 in $4\frac{1}{2}$. In the provincial hospitals the mortality is one in 4.

"The amputations of the leg for disease show a mortality of 1 in 27, which contrasts most favorably with the ordinary modes of amputating. In the London hospitals these amputations are attended with a mortality of 1 in 3 $\frac{1}{2}$, and in the provincial hospitals of 1 in 4."

For the purpose of further comparison, Mr. Teale gives in tabular order the amputations *by other methods* in the Leeds Infirmary during the same period of three years. Of these cases this is the summary :

24 Amputations.	Thigh . . . 2	Accident . . . 2	Death . . . 1
		Disease . . . 0	Recovery . . . 1
	Leg . . . 12	Accident . . . 6	Death . . . 0
		Disease . . . 6	Recovery . . . 0
	Arm . . . 5	Accident . . . 6	Deaths . . . 3
		Disease . . . 6	Recoveries . . . 3
	Forearm . . . 5	Accident . . . 5	Death . . . 0
		Disease . . . 0	Recoveries . . . 6
		Accident . . . 5	Deaths . . . 4
		Disease . . . 0	Recovery . . . 1
		Accident . . . 4	Death . . . 0
		Disease . . . 0	Recovery . . . 0
		Accident . . . 4	Deaths . . . 2
		Disease . . . 1	Recoveries . . . 2
		Accident . . . 4	Death . . . 0
		Disease . . . 1	Recovery . . . 1

and Mr. Teale's remarks are these :

"This series of cases, although they are not numerous, is interesting from the close resemblance of its results to those obtained at the London and provincial hospitals on a much larger scale.

"It is further important, as showing that during the three years over which our observation of the new mode of amputating extends, purulent and putrid poisoning of the blood prevailed. This fact was also too painfully shown by other cases as well as by the amputations.

"On referring to the table of amputations by the new method, it is seen that of 6 cases of amputation for accident, one death only occurred, and this death was not pyæmic, but was caused by shock; whereas, in 17 cases of accident, in which amputation by other methods was performed, in the same hospital, by the same surgeons, 10 died, and of these 10 deaths 7 were caused by purulent or putrid poisonings, as shown by their peculiar deposits in the viscera, or by unhealthy recent inflammation of membranous surfaces, more especially the serous."

The *character of the stumps* obtained by this method of operating is then considered. Their chief peculiarity consists in their having a soft mass of tissues, devoid of large nerves, moveable over the sawn end of the bone, which enables them to bear pressure on their extremity. This is a vital point, for in the ordinary circular and flap operations the cicatrix is adherent to the end of the bone, and, as Mr. Teale shows by the evidence of surgical mechanics in large business as makers of artificial limbs, the extremity of the stump is always intolerant of pressure. Of the cases in which the operation has been performed there are already 13 which show clearly that the stump will bear pressure.

"All these patients," says Mr. Teale, "are in full exercise, bearing a considerable portion of the weight of the body on the end of the stump.

None of them are thus bearing less than one half of the weight, some two thirds, and one the entire weight, in consequence of the limb having shrunk in size, and therefore moving loosely in the socket. All these patients are able to pursue their ordinary avocations for the full period of a day's work. One young woman can walk ten miles a day; and the youth, who bore the entire weight on the end of the stump, walked in this condition on one day thirty miles."

Upon this subject Mr. Teale adds:

"Having ascertained the capability of these stumps to bear pressure, I have endeavoured to determine the degree of it that is most favorable for easy progression; and the conclusion I have arrived at is, that it is best to divide the pressure pretty equally between the end of the stump and the upper part of the limb.

"I therefore recommend to the instrument-maker, in the first instance, to allow the stump fairly to touch, but not to press strongly upon, the bottom of the socket. The patient is then instructed to add to the bottom of the socket from day to day a circular piece of flannel, until he bears on the end of the stump fully one half of his weight. The pressure may, indeed, be gradually increased beyond this point, but such increase I believe to be unnecessary and undesirable."

After these general remarks, Mr. Teale proceeds to consider the particular mode of amputating the thigh, leg, arm, and forearm, and here the reader has all the help which he can receive from excellent woodcuts.

In conclusion, we would again express our belief that all surgeons will unhesitatingly adopt this mode of amputating as soon as they are acquainted with it, and we congratulate Mr. Teale upon this great addition to his high reputation as a surgeon.

On Ophthalmostasis, with an account of an improved method in extraction of the Cataract. By JOHN F. FRANCE, Ophthalmic Surgeon to Guy's Hospital.

After describing at considerable length the various contrivances—all of them unsuccessful—which have been resorted to at different times for the purpose of steadying the eye, Mr. France proceeds to say:

"For many years I have been accustomed to steady the eye during extraction by the contact and pressure of the fingers alone, according to the practice of most modern operators; the forefinger holding the upper lid, and restraining the globe's movements upwards, the middle finger on the caruncle curbing its movements inwards. In many cases this arrangement is sufficient for the purpose, and the section is made not only satisfactorily but with ease. In how great a degree, however, that ease is dependent on the patient's strength of nerve and steadiness of eye; and how limited the surgeon's real command of the globe is apt to prove, when the opposite qualities are manifested (especially if the anatomical conformation of the parts happens at the same time to be unfavorable), every operator of wide experience and equal candour must confess. Can no unobjectionable means then be devised which shall render his command absolute?"

"In operating for the formation of artificial pupil I first became aware of the practicability of holding the eye perfectly still and motionless, or as nearly so as possible, by the mere application of artery forceps. The idea at length was suggested of extending the use of this instrument to another operation, in which, as far as I know, it had never been employed (at least in this country) before; of availing, myself, in short, of the same resource as in cases of artificial pupil (and with a similar object) in cases of extraction.

"I have since brought the idea to the test of experience, with the result which it is my present object to make known—the result, that is, of facilitating in a degree I could not have anticipated the most critical stage of this operation. The mode in which I proceed is as follows. As soon as the patient is laid on the operating table and all the preparations are complete, standing at his head, I apply the extremity of the forceps with rather firm pressure a little beneath the inferior margin of the cornea, and clasp a somewhat broad portion of conjunctiva and of the submucous fascia securely. Then, taking the instrument between the finger and thumb of the other hand, as near as practicable to its closed points, I deliver it to the assistant; whose hand, supported upon the patient's cheek, receives it, and holds it as he would a pen. It is well that the assistant should be practised in his share of duty on the dead subject. The ordinary artery forceps are, on the whole, preferable to those with a spring catch, commonly known as Liston's; but it is of consequence that the nibs should be broad, and the teeth sufficiently prominent. The lower lid requires no further depression than that necessarily produced by the attachment of the instrument to the ball in this way. I then raise the upper lid with the fore-finger, direct the assistant to draw the cornea into a central position and retain it there with the forceps, place my fore and middle fingers on the globe in the usual way, and thus perfect the command of the organ. On now making the section, the eye is found steady and motionless; the knife can be deliberately entered, deliberately carried across the chamber, and deliberately brought out on the inner side of the cornea; and counter-puncturation being fully effected, and the flap on the verge of completion, the object of the forceps is accomplished, and they are at once disengaged. The remainder of the operation is finished in the ordinary manner.

"I have had much experience in the operation of extraction, having performed it myself considerably upwards of a hundred times; and of course am familiar with its pleasures (so to speak), and its difficulties, its contingent casualties, and the sequelæ of embarrassment attending them. After one or two trials, therefore, I was in a position to estimate the amount of advantage gained by the accessory manipulation just described; and this has proved indisputably so great, that I have employed the forceps without exception ever since. It will, perhaps, be the best way to place before the reader the facts which the current season has enabled me to gather, as practical criteria of the soundness of my conclusions.

"I have, then, since conceiving the idea that forceps would be beneficially available in this operation, performed extraction in the following cases: the recumbent posture; the superior section; generally

the previous application of atropine; Beer's knife; and the use of the right or left hand respectively, as the cataract was to be removed from the right or left eye, being adopted in all; several of the patients had cataract in both eyes, but that operated on alone is indicated."

Then after relating twenty cases in which he had put this method of operation to the test, Mr. France proceeds to say:

"The above examples comprise all the cases of extraction I have operated on during the present season, since adopting the use of the forceps; and the effect of the instrument has unquestionably been in an eminent degree advantageous. It has facilitated the operation in difficult cases, in exact proportion to their previous difficulty and risk. This was strikingly evinced in Cases 11 and 16; both of which patients had, at a former period, occasioned the utmost anxiety, from the impediments their restless eyes presented to the satisfactory performance of the operation. They, indeed, recovered their sight well, but it was in spite of dangerous obstacles. This year they each recovered the sight of the second eye, under circumstances strongly contrasted; that which was so embarrassing before was now effectually surmounted at the very outset, by the simple means we are considering. Still more remarkable in some respects is Case 17, that of the sailor; who on two previous occasions had proved so utterly uncontrollable, that I was compelled to desist from the attempt to operate. On the present occasion, with the aid of forceps, after a little preliminary trouble, the globe was brought into and held in a sufficiently favorable position for a good section to be made. Could more convincing evidence be afforded of the value of this appliance?"

"The assistance rendered by the forceps is further illustrated by the fact, that in not a single case did premature escape of the aqueous humour (with its attendant inconvenience of the iris folding over the knife, and forbidding the immediate completion of the section on pain of wounding that membrane) occur. In three cases only did the first incision prove from any cause inadequate, and resort to the bistoury become expedient; and it was in one of these alone (in manipulating the bistoury after the forceps were detached) that the iris was sensibly touched.

"I am well convinced that the advantages of the mode of operating now recommended will be fully appreciated upon trial; it may, therefore, be better for me to bestow a word or two in anticipation of any objection which might perhaps deter some one from the experiment. Really, the single objection which occurs to me as sufficiently plausible to merit notice (if the directions above given be followed, and especially that of disengaging the forceps as soon as counter-puncturation is complete, and before cutting out), is, that the conjunctiva might possibly suffer injury from the forceps sufficient to awaken troublesome inflammation, and compromise the result. A conclusive answer is afforded by experience: no mischief has, in any case, under my observation, ensued; and this fact is substantiated, not by the foregoing cases alone, but also by numberless cases of artificial pupil, in which forceps are now habitually used, as well as by many others of soft cataract (to which I have latterly extended their use), and which are all equally available for the determination of this point.

"It is right that I should acknowledge myself indebted to the work of Desmarres, already referred to, for the *idea* of adapting artery forceps as an 'ophthalmostat' in extraction, but not for their *advocacy*. He, in fact, discountenances their use, giving a preference it is needless here to impugn to Pamart's lance, and his own peculiar thimble. He appears to have employed the forceps (if at all, which is doubtful) with his own hand, instead of confiding them, as I have done, to another, and thus to have encumbered their application with a serious drawback. For he must, of necessity, in consequence, have committed charge of both lids to the assistant;* and so relinquished the twofold advantage of commanding the upper lid himself, and of aiding, with his fingers in the usual position at the lid and canthus, the influence of the forceps and the guidance of the knife."

Notes on the surgery of the War in the Crimea, with remarks on the treatment of Gunshot Wounds. By GEORGE H. B. MACLEOD, M.D., F.R.C.S. (Post 8vo, Churchill, 1858, pp. 439.)

The prominence which was from necessity given, during the continuance of the Crimean campaign, to the sanitary condition and medical details of our army, threw somewhat into the shade the surgical history of the war, and we possessed no connected account of the results of the surgery practised in the camp and hospitals before Sebastopol, until the publication of Dr. Macleod's work. In this volume there is given an admirably written and very valuable and instructive, and even full account of the surgery of the war, although, as the title intimates, a complete history is neither intended nor attempted by the author.

In three preliminary chapters Dr. Macleod treats of the physical character and climate of the Crimea, the sanitary condition of the camp and army, the diseases which ravaged the troops, and the campaign in Bulgaria; and these chapters present the most vivid and comprehensive yet correct medical history of the war that it has been our lot to read. In subsequent chapters Dr. Macleod treats *seriatim*, of the distinctions between civil and military surgery, of the peculiarities of gunshot wounds, of the use of chloroform in the Crimea, of primary and secondary hæmorrhage from gunshot wounds, of tetanus, gangrene, and erysipelas, of injuries of the head, of wounds of the face and chest, of gunshot wounds of the abdomen and bladder, of compound fractures of the extremities, of gunshot wounds of the joints, excision of joints, and of amputation.

Each chapter is enriched with the experience of the French surgeons, principally derived from personal intercourse with them, and Dr. Macleod contrasts the surgery of the Crimean war largely with the recorded experience of recognised authorities on military surgery at home and abroad; so that his work, in some respects, forms a species of running commentary, illustrated with recent cases, on the principles and practice of military surgery, and as such it must prove of no small interest to the

* This is evident from the words, "La pique de Pamard, a comme d'autres instruments nécessaires à la fixation de l'œil, le désavantage d'obliger l'aide à écarter les deux paupières" (vol. iii, p. 184); and is further demonstrated by the figure at page 200.

student and to the military and naval surgeon. An appendix, consisting mainly of the surgical statistics of the war, is added to the volume.

The following extracts, referring to two of the most important questions of military surgery (one dating, with our own army, only from the time of the Crimean war) will, apart from cases, give a notion as to the character of the strictly surgical portion of Dr. Macleod's work.

Effects of the conical ball.—"The great velocity, peculiar shape, and motion of the conical ball, give to its wounds a character considerably different from those which is present in wounds caused by a round musket ball. If fired at short range, and if it strikes a fleshy part, the conical ball produces, I think, less laceration of the soft parts than the old ball; but if the range be great, and the part struck bony, with little covering of flesh, as in the case of the hand or foot, then the tearing, especially at the place of exit, is greatly more marked.

"I have not been able to satisfy myself in all cases, so clearly as the description of authors would lead me to suppose that I could, as to the characteristics which distinguish the wound of entrance from that of exit. That the former is more regular and less discoloured than the latter is true in many cases, but that the lips of one wound are inverted, while those of the other are everted, has seldom been clearly marked to my observation. If the speed of the ball be great, and no bone have been struck; then there is little difference in either the size or discoloration of the wounds; but if the flight of the projectile be so far spent as to be retarded by contact with the body, especially if it have encountered a bone or strong aponeurosis, so that its speed is considerably diminished before it passes out of the body, then the wound of exit will considerably exceed in size that of entrance. This is especially true of conical balls. If, on the contrary, the ball be fired close at hand, so that its speed is not sensibly diminished by its passage through a limb, then the difference of size will be very small, and may even be in favour of the wound of entrance, as I had twice an opportunity of observing."

"There cannot be a doubt that the old round ball, if fired at a certain range, comminuted bone, but it is equally certain that at a longer or shorter distance it frequently failed to do so.* When fired a few hundred yards off, it had hardly force to enter the body, but might be diverted, as it has been, by the point of the nose. If it did enter, and impinge upon bone, it might only dent it, as may be seen by an example in the Musée Dupuytren, in Paris; or it might groove it merely, or penetrating the substance of the bone, it might remain at rest without splitting it, as can be verified in any museum of a military hospital. If fired, again, at close range, the round ball might go through a bone, making a bore as clean and sharp as if formed by a punch. Of this fact many illustrations can be seen in surgical museums. Now, so far as my observation goes, none of these results follow the stroke of a heavy

* I believe that the proportion of cases in which balls have passed through the fleshy parts of an extremity without fracturing the bone, will be found to be much less in the Crimean than in other wars. Thus, in one series of cases mentioned by Deputy-inspector Franklin, in his report on the wounded at Meanee, thirty-one cases of matchlock wounds of the upper arms are given, and in only one was the bone broken. To work out this point in figures so extensive as to be of any use, would require details not supplied by writers on the old wars.

conical ball, such as that used by the Russians, at whatever range it is fired. It never rests in a bone, channels, or perforates its substance, without splitting it, like a wedge; nor does it ever come to mark a bone with any touch more gentle than what occasions its utter destruction. In the Crimea we had many opportunities of observing the action of both kinds of ball, and so far as I could judge, their effects were so dissimilar, as almost to justify a classification of injuries founded on the kind of ball giving rise to them. The longitudinal splitting of the bone is so dexterously and extensively accomplished by these balls that, while but a small opening may lead to the seat of fracture, the whole shaft may be rent from end to end. I have repeatedly seen the greater part of the femur so split. Stromeyer has shown that this longitudinal splitting seldom transgressed the line of the epiphysis, an observation which I can most decidedly confirm; for though the injury has at times been sufficiently severe to implicate both, yet the rule has been just as he says.

“Gunshot fractures of the extremities of bone have always been considered dangerous, chiefly on account of the shock, the comminution of bone, and the fact, that the wound leading to it is of such a character that it can heal only by suppuration, and cannot be so closed as to convert it into a simple fracture, which, it is well known, we can sometimes accomplish in such fractures as present themselves to us in civil practice. The cavity of the fracture is thus kept open to the air; the pus undergoes those changes which Bonnet has shown it does under such circumstances, and that severe and prolonged inflammation of the deep and irritable tissues which constitutes the chief danger in compound fractures, cannot be avoided.* Now, all of these dangerous characteristics of compound fractures have been immensely increased by the conical ball. First of all, the shock it occasions is undoubtedly greater than that caused by the round ball, simply because the destruction it causes is much more severe; secondly, the comminution of bone is enormously increased. The number of fragments which are quite detached are much more numerous, and the amount of sequestra, which are so far severed as to be ultimately thrown out before a cure can be looked for, is much greater. Thirdly, the bruising of the soft parts is more extensive, so that the suppuration is more prolonged, and the changes of purulent absorption so much the more multiplied.

“The great loss of substance which follows compound fractures by the conical ball, is the source of one peculiarity in their treatment. The

* “All the complete fractures of the other bones of the extremities unite when they are well managed; by what fatality are those of the femur not equally fortunate? Is it the diameter of the cavity of the bone; the quantity of medullary substance which it contains; the peculiar structure of the vessels which carry the nourishment; the size and force of the muscles which are attached to it, which, by their weight and pressure, obstruct the passage of the liquids? All these causes united may combine together, and give rise to that want of success which we meet with in treating complete fractures of the femur caused by fire-arms; but complete fractures of this bone heal very well, whatever cause has produced them, when they are not accompanied by a wound. These reflections, which the bad success of those cruel fractures has suggested, have caused me to present to the public in 1750 a method for amputating the thigh at the hip, and that to try and snatch the wounded from inevitable death.”—Ravanton, *Chir. d'Armée*, p. 324.

shortening will be greater should consolidation follow, than if the injury had been occasioned by the round ball. The conviction has been strongly impressed upon my mind, by the observation of not a few of these cases, that we ought not to keep up extension in their treatment, except in a very modified degree."

Amputation.—"Military experience on this point must regulate military practice, and the results of civil experience must continue to regulate civil practice. To military surgeons, the question of primary or secondary amputation is a settled one. The experience of every war has more and more confirmed the advantages of early operation, and that in the Crimea has not disturbed the rule; in fact, later observation would lead us to go further, and in place of merely advocating interference within twenty-four hours, the prevailing idea at present would be better expressed by saying that every hour 'the humane operation' is delayed diminishes the chances of a favorable issue.

"It is impossible to prove from any returns the full bearing of this question, as the mere number who survived after a given number of operations, performed primarily or secondarily, by no means expresses the terms of the question. It would manifestly be necessary to know how many died before the second period came round, and to these should be added the victims of delayed interference, with all the pain and suffering which such delay occasioned, before we can arrive at a just estimate of the results of either proceeding. The experience in the Crimea in favour of operation was unequivocal in both armies, and needs no illustration from me.

"Chloroform has done much to render the success of primary amputation, as contrasted with secondary, yet more marked. If we believe, as I certainly do, that by the use of this anæsthetic all fear of intensifying the shock is obviated—which was one reason why surgeons delayed operation—then the tendency of military surgery, since the introduction of chloroform, must be to still earlier and more prompt interference.

"Secondary amputations were much more common during the early than the late period of the war—a circumstance which arose from the deficient means of treating the wounded in the camp during the former as compared with the latter period, and thus the necessity that existed of despatching them from camp immediately after being injured; and this, together with the better hygienic condition of the patients towards the end of the war, accounts for a fact, well known to those who served in the East, but which the range of the returns does not enable me to show in figures, that amputations were much more successful as a whole, towards the conclusion, than at the outset of the war. At first, too, when patients were early sent from camp, not a few operations, to my own knowledge, were performed during the 'intermediary' period, and, without one exception, those thus falling within my observation were fatal."

"In considering the statistics of amputation performed during the Crimean war, the figures refer solely to the period between the 1st of April, 1855, and the end of the war, and consequently exclude all the unfavorable part of the campaign, as well as the greater number of the operations which were absolutely performed during the war. It was found impossible to attain to accuracy with regard to the early period,

so the field of observation was restricted as stated. It is needless to point out how different must be the lessons derivable from the statistics of this latter period alone, to what they would have been if the whole period of the war had been included.

"During the limited period I have mentioned, there were 732 amputations in all parts performed, followed by death in 201 instances; of these, 654 operations and 165 deaths were primary, and 78 operations with 36 deaths, secondary; giving a per-centage of 27·4 deaths overhead—25·22 for the primary, and 46·1 for the secondary operations. If we include only the greater operations, viz., amputations of the shoulder, arm, and forearm, of the hip, thigh, knee, and leg, then we have a total of 500 cases and 199 deaths, or 39·8 per cent.; of which total 440 cases and 163 deaths, or 37 per cent., were primary, and 60 cases and 36 deaths, or 60 per cent. were secondary.

"The increase of the mortality as we approach the trunk may be shown thus, taking the primary amputations alone as giving the most unbroken series:

" SUPERIOR EXTREMITY.	
Part	Ratio mortality per cent.
" Fingers	0·5
" Forearm and wrist	1·8
" Arm.....	22·9
" Shoulder-joint.....	27·2
" INFERIOR EXTREMITY.	
" Tarsus	14·2
" Ankle-joint	22·2
" Leg	30·3
" Knee-joint	50·0
" Thigh, lower third	50·0
" " middle	55·3
" " upper.....	86·8
" Hip-joint	100·0

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